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AMERICAN STATISTICAL ASSOCIATION

JUNE 1948

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American Statistical Association

Organized November 27, 1839
Incorporated 1841

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JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION

Number 242 JUNE 1948 *Volume 43*

THE WAR PRODUCTION BOARD'S STATISTICAL REPORTING EXPERIENCE

DAVID NOVICK,*
University of Puerto Rico
AND
GEORGE A. STEINER,†
University of Illinois

This is the first of a series of four articles that will present an exploratory analysis of the experience of the War Production Board and its predecessor agencies in developing and using data from industry for policy and administrative purposes during World War II. The present article describes the growth of the WPB's reporting structure.

INTRODUCTION

INTELLIGENT wartime industrial control is impossible without adequate methods for collecting and using facts. In establishing policy for control over industrial production and distribution and in administering these controls, it is necessary for government continuously to make choices among competing demands for resources at all levels of economic activity. Facts and factual collection methods are managerial tools without which intelligent choices among alternative courses of action cannot be made.

When the occasion arose for the national government to direct the industrial machine of this nation in World War II, there existed neither the data necessary to support basic policy decisions and administrative action nor the technical statistical discipline by means of which the data could be produced. The task of industrial mobilization and continuous control of the American industrial machine during the war was unbelievably complex and difficult. But it was a problem immeasurably complicated by the absence of an efficient emergency industrial statistical reporting system.

* Formerly Director, Program and Statistics Bureau, War Production Board.
† Formerly Director, Industrial Statistics Division, Civilian Production Administration.

In 1940 and 1941 it became clearer daily to those administering the industrial production program that neither the necessary facts nor satisfactory methods for getting them were available. Peacetime industrial statistical series were, for purposes of wartime mobilization, incomplete in coverage, not timely, deficient in penetrating detail, subject to serious inaccuracies, and uncoordinated. In addition, peacetime methods of collecting and analyzing industrial series in many instances were found inadaptable to the needs of war.

These deficiencies, although lamentable, are understandable. Peacetime factual needs, and methods and series to support them, are far different from those required for wartime administration. In time of peace the economy of this nation is not a centrally directed one but rather one in which the totality of economic activity is based upon a multitude of individual transactions free of government restrictions except those of a general policing character. As a consequence, no need exists for the government to collect data which reflect detailed economic actions. Rather, government is concerned more with information about over-all economic activity and data necessary to carry on specific administrative programs of various regulatory commissions and boards.

This type of data and many of the methods used to collect them are not those required by war. In periods of crisis, national policy demands that the sum total of business operations accomplish specific and well-defined objectives. To meet these objectives the government is forced into day-to-day relations with business units. Government administrators, as a result, need to collect data showing a balanced factual picture of the industrial machine, in which each detail is related to the total and to every other. Business management requires a continuing flow of accurate and detailed information to make the kinds of decisions needed to conform to government regulations and to effect the maximum contribution of the concern's resources to the national objectives. These are crisis needs.

Prior to 1940 the Federal government had two experiences with factual needs of crisis administration. As part of the World War I program of industrial control, the Federal government found it necessary to introduce a large variety and number of industrial data reporting systems. These provided information necessary for the formulation of policy decisions and administrative actions relating to the distribution and production of specific materials and products. At the end of the war practically all of the wartime reporting structure was abandoned, and many of the lessons were buried with the dead files of that conflict.

When the national economic machinery broke down in the 1930's, the

government found it necessary to expand its concept of public welfare to include both control over and direction of private business so that the net effect of business operations would be centered upon economic recovery. As an inevitable part of the regulations of such emergency agencies as the National Recovery Administration and the Agricultural Adjustment Administration, the government again developed a complex industrial statistical reporting system by means of which it could support basic policy decisions and administrative actions relating to business operations. With the invalidation of the NRA and the AAA, the data collections were not continued by either government or industry and no study of emergency statistical methods was undertaken.

As a result, there was not available at the beginning of World War II the industrial statistical reporting discipline that emergency administration was found to require. Starting from this point, industrial mobilization administrators began through experimentation, improvisation, and trial-by-error methods the laborious and costly task of constructing an industrial statistical reporting framework suitable to meet emergency management needs. It was only through bitter and time-consuming struggle that the War Production Board eventually developed methods and techniques by which its comprehensive reporting requirements were reasonably adequate to meet its most urgent policy and administrative needs.

In a matter of days following VE-Day the great majority of the WPB reporting instruments were discontinued, as they should have been. In peacetime years the government avoids day-to-day operating relationships with business. This also is as it should be. As a result, both the wartime statistical series and the methods and techniques developed to acquire and use them will tend to disappear. Undoubtedly some of the wartime experience will be incorporated into industrial and governmental peacetime statistical collections and methods. But the integrated discipline of a satisfactory wartime industrial statistical reporting system will find no peacetime use.

This inevitable peacetime disuse must not result in unpreparedness for a possible future emergency. Adequate preparation must embody preservation of the principal lessons of World War II concerning factual tools and procedures required to manage successfully the economic system. But merely preserving the lessons of World War II experience is not enough. Many important tools of administration were still crude and blunt at the end of the war despite the whetstone grinding of five long war years. In time of peace these tools must be studied and improved so they will be razor-sharp for immediate use in any emergency.

Study of the experiences of the WPB in collecting, analyzing, and using industrial statistics, if executed at the proper level of professional proficiency, can save the nation both millions of dollars and years of time in improving its peacetime industrial reporting systems. Perhaps of more importance, neglect to take advantage of and build upon this experience will more than likely leave the nation, on the impact of any future emergency, without the factual tools required to administer economic matters. Failure to have available for immediate use in a future crisis an adequate reporting system may be catastrophic if the military movements in that emergency involve the sudden devastation now attributed to atomic bombs, supersonic projectiles, and guided missiles.

The authors have tried to avoid the use of footnotes. The great bulk of source material, aside from individual questionnaires, is located in personal memorandum of the authors and memorandum of various WPB officials. Both sets of documents are generally unavailable. For those interested in pursuing further the analysis of methodology evidenced in individual questionnaires, attention is drawn to the *Catalog of War Production Board Reporting and Application Forms*, prepared by the Industrial Statistics Division of the Civilian Production Administration in 1946. This *Catalog*, published in ten volumes by the U. S. Government Printing Office, reproduces and analyzes approximately 1,200 of the WPB's most important questionnaires.

PART I

GROWTH OF THE WPB REPORTING STRUCTURE

To satisfy the needs both of policy-makers and of administrators for information to control production and distribution, the WPB created and used a reporting structure of unprecedented scope and complexity. This reporting structure was not built. It was assembled. It grew out of the needs of the moment; it was altered to reflect rapidly shifting objectives, organization, policy, and personnel; and it was molded from the compromise of powerful opposing influences. The problems encountered in its development were as complex and broad as the industrial production system it was designed to harness. No single discipline or procedure ever animated all of its supposedly systematic parts. Its techniques were never completely mastered and its potentialities never fully realized. At no time did it fully satisfy the fundamental needs of policy and administration.

The purpose of this section of the series is to reduce this vast and

intricate agglomeration of fact-gathering devices to understandable proportions and by so doing to establish a background which may serve for a presentation of selected fundamental technical problems and lessons connected with WPB statistical methods. To accomplish this, there is presented a broad panoramic sketch of the principal movements taken by the reporting structure through the war period. It is an analysis of the struggle to develop a reporting system to produce facts for wartime management, the principal problems faced in the struggle, and the attributes of the methods introduced to reach the goals set.

Closing Informational Gaps in 1940. In the year 1940 the day seemed remote when the United States would have to reconstitute its industrial system into a single-purpose war-production machine. Defense production for our Allies and our own armed forces at first was comfortably accomplished within available productive resources. Only gradually and in individual areas did it encounter resistance. As a consequence the importance of an adequate statistical discipline to support industrial mobilization management became apparent rather slowly. Actions taken to develop a sound statistical order were at first, therefore, essentially in the nature of stop-gap measures.

The first comprehensive factual research by the National Defense Advisory Commission was designed to establish broad industrial mobilization policy guides. Efforts were made to develop facts about the magnitude of economic resources available for war and nonwar demands, the level at which specific resources might cease to be adequate to satisfy potential demand, and the industrial and geographical areas in which new plant facilities should be built and located. Existing data proved to be inadequate for this work even though completion of the studies required only the broadest type of information.

Failure of this approach led to another which turned out to be even less fruitful. Military agencies were urged to develop complete statistical pictures of their procurement and the claims which they were currently making and contemplated placing in the future on scarce materials and products. This, together with a comparable statement of nonmilitary demand, it was correctly reasoned, could provide much of the basic information needed for broad industrial mobilization policy decisions. By the time useful action was taken in developing such overall statements of claims on industry, the United States was already engaged in war!

The earliest demand for detailed industrial data centered upon plant operations and capacity. Recognizing the paucity of figures in government about such matters a twofold effort was made to get facts from

industry. First, various State Industrial Councils obtained information on plant capacities and equipment within their states. Second, the Bureau of Labor Statistics was asked to develop a report on individual plant operations by which the NDAC could learn the extent to which plants were actually operating 24 hours a day, seven days a week. The State Councils accumulated a series of reports and submitted them to the NDAC but the results were inadequate for solving the statistical problems of that agency. The Bureau of Labor Statistics developed a questionnaire which requested monthly reporting from about 300 plants in the metal-working trades concerning man-hours worked, labor shifts, and weekend employment. Both programs fell far short of the factual requirements of the times.

Hindsight reveals quite clearly that such amateurish methods could not provide the information required for a war. There was, however, no dearth of suggestions for getting the information needed. In the spring of 1940 and continuing into the autumn, for example, there was considerable discussion in the NDAC of the desirability of collecting from industry highly detailed reports as a part of what was vaguely referred to as "a modified German Plan." It was assumed that the Germans, who were much deeper in war than the United States, had developed a machine by machine, man by man, pound of material by pound of material, weekly or daily reporting system which by some type of tabulating magic was quickly converted into both an all-out summary for policy-making and a completely detailed report for administrative decisions. The apparent success of German industrial mobilization led to the idea that the United States should have the same sort of statistical system. Introduction of this type of reporting system, however, inevitably would have been labeled a "bureaucratic deluge of paperwork" at this stage of the war effort. For this and other reasons the idea was abandoned.

Individual Preference Ratings. The search for information could not be allowed to delay the solution of current problems concerning acquisition of scarce materials needed to produce vital defense equipment. To meet these issues, the Army and Navy Munitions Board, with but limited legal and executive authority, established in August 1940 a priority plan based on preference-rating certificates. By means of these documents (PD-1, 2, 3, 4 and 5) it became possible for producers holding government contracts for defense equipment to apply for, receive, and extend preferred treatment to items they and their suppliers needed to fulfill their contracts. A *Priorities Critical List* was established to limit the materials and products to which preference could be ap-

plied. The listing was relatively short and specified items in broad, general terms, such as: barometers, bombs, cranes, explosives, chromium alloy steel, aluminum and aluminum alloys, and tin.

With a broadening and acceleration of defense production and expansion in the materials and products for which preference was applicable, this system operated less effectively with each passing day. So long as sufficient materials and productive capacity existed to support both defense and unrestricted civilian production the system was effective in assuring to manufacturers with government contracts the materials they needed for production. But, as defense production mounted, it became increasingly difficult to control preference ratings so as to hold preference-rated demand for critical materials within available supply. The inevitable result was the rise of log jams among competing authorized demands in more and more products and materials.

These log jams could not be broken by the individual preference-rating system. The system contained no automatic adjudication of competing claims at any level of manufacture when supplies of materials and products were insufficient to fill all rated demands. In addition, the rating certificates could not be used to produce the facts government could use to resolve the problems by administrative action. Not only was there little uniformity in data incorporated in rating certificates, but the flow of paper reached such proportions that it was impossible to use the data for comprehensive policy or broad administrative action.

Breaking the Log Jams. The first efforts to break these log jams followed admonitory rather than mandatory patterns. Policy decisions made on the basis of whatever data were available were translated into detailed regulations and control orders for which there was no implementation in the form of reports transmitted between government and industry. It was only a matter of months, however, before problems in connection with such tight items as nickel-bearing steel, magnesium, aluminum, machine tools, and heavy industrial equipment became so acute that the inadequacy of generalized self-administered controls to effect the kind of production and distribution control required by the situation was generally recognized. Experience demonstrated that only the most detailed type of administration and centralized control could be expected to provide the desired results.

In the autumn of 1940, the need for new detailed information to solve critical problems connected with individual products and materials had become so urgent that a fresh approach to the acquisition of data was of paramount importance. Each of the Divisions of the

NDAC,[1] and later the Office of Production Management, charged with responsibility for control of a particular material or product worked out new and more detailed programs for the collection of data required for daily administration of production and distribution controls. Those responsible for magnesium, for example, sought data about all the direct and indirect economic problems involved in the production and use of magnesium. The same pattern was followed by those responsible for nickel, alloy steel, machine tools, and other items in short supply. Centralized control over questionnaire issuance fortunately had been introduced in the early days of the NDAC and continued into the Office of Production Management. As a consequence, surveys actually introduced as a result of these programs were limited to those most obviously needed to solve an immediate problem. The country was thus spared the painful experience of innumerable data requests which inevitably would have overlapped, duplicated, and in other ways interfered with one another, not only in the data collection itself but also in the analysis and administration of the results. A trend was begun, however, to acquire more and more data about individual products and materials, each with little reference to the other.

In March 1941 a more realistic approach was made by the recently formed OPM (Office of Production Management). This involved modification of the preference-rating system by the introduction of P, or "blanket preference rating," orders, providing preference-rating assistance to classes of products; and M and E orders, directing the distribution of basic raw materials and equipment. In September 1941 L orders were introduced to facilitate operation of M orders by limiting nonessential demand for materials under M-order control. The great bulk of information pertaining to individual products and materials which was acquired throughout 1941 accompanied administration of these orders.

P Orders. P orders provided for the assignment of ratings to classes of products, usually at the final product end of the industrial system. One type of P order permitted automatic assignment of ratings for procurement of material needed in the production of the classes of end products specified in the order. Such were the first orders, namely, P-1 applicable to electric traveling cranes, P-2 applying to machine tools, P-3 for airframes, and P-4 for airplane engines and propellers. Each

[1] Throughout the text all subdivisions of various agencies are specified as Divisions, for purposes of clarity, except in a few instances in which quotations from orders incorporate the official title of a subdivision which is other than a division.

order, with few exceptions, specified a reporting form upon which producers to whom orders were issued had to report periodically to the OPM. In addition, producers using or receiving preference treatment under an order were required to maintain records as specified in the order. This provision, however, was often vague and carried no compulsion of compliance. This type of P order, on the whole, was more successful in relieving a tottering preference-rating system than in developing satisfactory new quantitative measuring tools and methods for using them.

The Defense Supplies Rating Plan, issued under order P-6 in May 1941 was a different type of order and its methods were destined to become of vast importance in WPB operations. Certain manufacturers, such as those engaged in producing electrical motors, twist drills, and mill supplies, received defense orders indirectly and on shorter delivery cycles than their manufacturing time. Deliveries customarily were made from stock, and replenishment of scarce materials for production of additional stock could not be made by extensions of preference certificates for the small amounts of separate materials going into each individual motor or twist drill. The application of individual preference ratings to these producers was quite unrelated to the manufacturing procedures of their products. It was the purpose of P-6 to facilitate production for such manufacturers.

Each manufacturer applying for aid under the order presented to the OPM, on form PD-25, the following information for his plant or subdivision thereof; past quarterly material consumption, material stocks on hand, and current quarterly requirements for those scarce materials for which assistance in acquisition was requested; the amount of defense business; and the specific finished products for which scarce materials were needed. This plant questionnaire embodied rudimentary methods from which profitable development was to be made.

Principles originated by the Defense Supplies Rating Plan were used again in August to aid manufacturers of various health supplies. Order P-29, Health Supplies Rating Plan, was issued to aid manufacturers of specified health equipment who were encountering difficulty in obtaining ratings sufficiently high to procure basic materials needed in their manufacture. Form PD-79, used to administer the plan, required from manufacturers plant reports which requested data about finished products to be made from scarce materials, quarterly consumption of and requirements for scarce materials, inventories of scarce materials, and withdrawals from stocks. With these data, as with the information acquired under the Defense Supplies Rating Plan, the OPM was in a

position to appraise the aggregate needs of each plant and to issue ratings accordingly.

To broaden the scope of the Defense Supplies Rating Plan, order P-90 was issued in December 1941 to launch the (voluntary) Production Requirements Plan. Its application form, PD-25A, represented a more advanced development of both PD-25 and PD-79. But data requested on PD-25A were also designed to enable a manufacturer to present a complete picture of his operations in relation to the war program and to essential civilian needs, and to state his production-material requirements for these purposes. So successful was the operation of this plan that by early 1942 approximately 7,000 plants were obtaining priority assistance under it.

These plans operated on the basis of an individual manufacturing unit. They recognized the fact that industry was made up of segments of companies and that producers were usually engaged in the manufacture of more than one class of products. The plans, therefore, were designed to provide priority assistance on a plant basis to facilitate procurement for a plant. Methods designed to acquire facts for these purposes were important in paving the way for future comprehensive plant reports.

Rise of Individual Material Allocation Systems. In March 1941 the same month P-1 was issued, the first M order appeared "to conserve the supply and direct the distribution of aluminum." It was followed in succession by M orders for magnesium, ferro-tungsten, tungsten metal powder, neoprene, nickel-bearing steel, and numerous other critical basic materials. (E orders directed distribution of finished products, such as machine tools.) These orders embodied the idea that control of the distribution of basic materials through the industrial system held the key to maximum war production. To administer a system incorporating this idea, producers of basic materials were required to file reports, usually monthly, requesting authorization to ship mill shapes and forms to customers. These reports requested detailed data on material shape and form production, orders by individual customers, and the products for which materials were ultimately to be used. Upon the basis of this information, administrators responsible for materials under control distributed available supplies among competing programs and products.

The dominant characteristic of control policy and industrial reporting in 1941 centered about hundreds of little universes circumscribed by these orders. Each system in use followed a rough pattern but so differed in details from other systems that uniform control and statistical reporting were completely absent.

The steel plate allocation system represents a reporting system designed to control the flow of semi-finished products through a fabricating bottleneck. As eventually developed, the reporting system required each consumer of steel plate to list his requirements with his supplier one month in advance, specifying government contract number, use of the material, the preference rating, and the order quantity. A copy of this form (PD-298) was transmitted to the Steel Division, together with a statement of steel plate consumption and inventory (form PD-299). The steel plate producers, in turn, listed and sent to the Steel Division (form PD-169) all orders received and specified those which were included in the rolling schedule for the following month. The Steel Division surveyed these orders and rolling schedules to determine whether any important military requirements had been omitted. In addition, an effort was made to review the schedules to insure the full utilization of the productive capacity of the steel plate mills. Reporting and analysis were centered only upon the needs of the Steel Division to make its decisions. These decisions were made in the absence of complete information about the status of aluminum, copper, and other materials, competing requirements in other areas, component schedules, assembly schedules, etc.

Most individual allocation systems operated on the basis of detailed reports from industry. The typical material report form, or application for authority to ship raw materials, requested all or part of the following type of information from a material producer, on a monthly basis: (a) the part into which the material was to be incorporated; (b) the product into which the part was eventually to be assembled; (c) the use to be made of the product; (d) the government contract number, if any, of the finished product; and (e) other information, such as preference rating and end-use pattern. Producers generally submitted this information on a series of forms. All orders provided for a report, some for more than one. In addition to producer reporting, allocation systems frequently provided for reports from consumers interested in acquiring raw materials under control, warehouses stocking materials, dealers, and others through whom the material passed in its metamorphosis from crude form to final product.

Among all the competing allocation systems there ran the thread of end-use. Most producers' reports requested data on the use which industry expected to make of raw materials, e.g., carbon steel bar to be made into a valve stem for a specific type of valve for a destroyer. Analysis of various forms created for the most important critical materials shows that there were as many different end-use reporting requirements as forms. Each differed from the other. The fact of nonintegra-

tion and nonuniformity of end-use requests was negligible beside industry's inability to supply the desired data. Most of the information requested was known only to the assembler of the finished end product, and only a well-established end-use reporting mechanism could transmit the information through the contractual chain. But no such transmission mechanism existed.

Each allocation system, operated autonomously by each Division, resulted in unbalanced flows of materials. Quantities and delivery dates authorized by the Steel Division might not bear any relationship to those authorized by the Copper Division, because neither the mills nor the Divisions consulted one another about the coordinated needs of the purchaser. Nor were they in any position to do so. Each group responsible for a material or product attempted to appraise the needs for its controlled item alone and to direct the use of its supplies through the industrial system in terms of what were considered to be priority needs, with little reference to operations among other allocation systems.

Although M orders provided better control over the distribution of scarce raw materials than preference ratings alone, they did not provide the balance that only a comprehensive over-all reporting system could achieve. Each group responsible for administration of an M order generally was willing to acknowledge the need and advantage of an aggregate statement of demand and supply covering all materials and products. But when this intellectual willingness was pressed with the suggestion that individual controls and specialized allocation systems should be abandoned in favor of aggregate reporting and over-all allocations, the bridge could not be crossed. The proposal was sacrificed to personal desires and individual notions of how control should be exercised. No support for over-all comprehensive reporting and allocations was given by those who were skilled in their specialized fields and who had erected their own control structures. Indeed, to a very large extent any coordinated effort to develop comprehensive controls was quickly torpedoed by the managers of the individual schemes, who saw in aggregate reports a diminution in their individual importance as controllers of segments of the economy. The maintenance of each individual control, they of course felt, was "absolutely essential to maximum production."

These personal feelings cannot be construed as indictments of the patriotism of these men. On the contrary, these administrators were extremely patriotic in much of their desire to cling to the control systems they had established. They knew their materials and they knew

the industries through which they moved. They were afraid that persons with less specialized knowledge might jeopardize the national interest not by malice but by ignorance.

Increasing difficulties in hammering down urgent demands for critical materials to match supplies led, in September 1941, to the introduction of L orders. L orders were basically designed to restrict production of certain classes of products to a stated quantity in a given period of time and to limit or prohibit the use of certain specified materials or products in the production of other products. They were preeminently a device by which less essential demand for products and materials under M-order control might be easily eliminated. L orders generally provided for industrial reporting, and as a result were fruitful sources of information about the products and materials to which they applied. Issued in limited numbers in the fall of 1941 (there were 26 issued up to January 1, 1942) they increased rapidly after the end of the year and by June 1942 numbered 154.

Reporting instruments accompanying L orders varied considerably since the specific provisions of different orders pertained to specialized problems of product and material limitations. Some orders required no reporting or applications; others used but one instrument; and some used a number of reporting forms. L-117 which limited production and distribution of heavy power and steam equipment, used (in August 1942) ten different statistical and application forms; L-41, pertaining to construction, utilized eleven reporting instruments. Through such reporting instruments the WPB was able to acquire considerable detailed information about the production, inventories, distribution, and use of products and materials covered by L orders.

Comprehensive Inventory Surveys. The only industry-wide report introduced in 1941 grew out of problems connected with inventory control. Early in the national defense program, manufacturers were impelled by every sound peacetime operating criterion to accumulate inventories of basic materials. By 1941, a point was reached at which a balance had to be struck between increasing defense materials requirements on the one hand and efficient utilization of tightening material supplies on the other. The uncomfortable spread between material supplies and defense requirements for them, already existing in the case of a few basic materials, threatened to expand rapidly to many materials. Facts about use and consumption of inventories, beyond data obtained through operation of M orders, were essential to permit government to flush out idle stocks to help restore balance in the materials demand-supply equation.

Combating this situation demanded facts—facts collected on a comprehensive, industry-wide basis. But again, half measures were tried first. In the beginning, the OPM limped along with spot inventory surveys of a few tight individual materials and the broad data available in the records of permanent government agencies. None of these surveys, however, provided systematic coverage of inventories from primary producers through various processing stages to final fabrication. But the data at hand, piecemeal and fragmentary though they were, showed that inventory accumulation was reaching serious proportions. There was little in them, however, to support specific policy decisions and administrative action to cope with the situation.

The first effort to solve the problem, through General Materials Order No. 1 issued in May 1941, was completely ineffective. Under the provisions of this order, which covered 29 materials, users of the materials specified filed a monthly statement (PD-19A) that they were not increasing inventories, and producers filed statements (PD-19B) that they were not shipping to idle inventories. The deficiencies of this order quickly became apparent, and other measures had to be taken to make operative the ideas embodied in it.

To establish a factual basis upon which General Metals Order No. 1 could be made effective, form OPM-69 (inventory and consumption of scarce materials) was sent in August 1941 to all manufacturers who were holders or users of any of the 70 scarce metals enumerated in the form. A similar form, OPM-75, was tailored to fit the needs of all dealers in and distributors of scarce metals. In addition, comparable information was required on forms OPM-132 and OPM-133 from financial houses, public utilities, and other groups who held these metals. OPM-69 was mailed to about 70,000 manufacturers; the mailing list for the other related forms aggregated approximately 30,000 names. This was the first industry-wide reporting effort in the war period.

Those to whom the forms were sent were asked to report the quantity of each metal which they had used in August 1941 and the amount on hand at the end of that month. Definitions of metals for which data were requested were broad and extended to the point where the raw materials lost their identity as raw materials and became fabricated products. Fabricated product definitions were also vague and inadequate.

The immediate results of OPM-69 and its related forms were rather disappointing, largely because of inexperience in the development of the form itself and problems connected with its tabulation. Tabulated summaries of OPM-69, as a result, were delayed until December 1941. In

the meantime events had moved so rapidly that the data reported on OPM-69 and its related forms were considered obsolete by the policy-makers and administrators for whom they had been collected. Not only had difficulties encountered in production moved far beyond the point where inventory accumulation problems were of dominant concern, but the Divisions of OPM had collected inventory and other production information for many individual products and materials on a current basis. Although the OPM-69 data soon lost currency, they contained much information which proved to be useful for a long time to come.

The methods of OPM-69 and its related forms, however, were significant at this stage of the war effort. Of most importance, the issuance of these forms broke the ice, so to speak, in the area of comprehensive reports connected with metal usage and consumption. In addition, much was learned about such technical problems as definitions, units of measure, instructions, mailing lists, and tabulations as they related to industry-wide reporting. Equally as important as the positive methodological lessons were the negative ones which taught what not to do.

Tabulating Preference Rating Certificates. Urgent needs for comprehensive information about many aspects of industrial production centered attention upon the aggregation of data contained in individual preference-rating certificates and resulted in a tabulating effort more ambitious than fruitful. These certificates contained data on materials used in production, manufacturer shipments of defense products, preference ratings, orders, and other sorely needed facts. For a considerable period of time the small volume of these certificates, centrally accumulated in the OPM, was hand-tabulated with some success, but little or nothing was done with those received by the Army and the Navy. As the flow of paper mounted, the Bureau of the Census was asked, in August 1941, to tabulate data by mechanical punch card methods. Thereafter, the flow of paper reached flood proportions and as a result tabulation difficulties grew faster than solutions. Completed summaries were never made and the project was abandoned in January 1942.

The Reporting System at the Time of Actual War. The net effect of all these developments was to leave the United States upon the eve of its active entrance into World War II without the basic statistical-administrative tools needed to cope with pressing production and distribution problems. The core of the most serious problems was demand-supply imbalance for metals. Throughout the fall of 1941 it was becoming more apparent daily that the supply of a number of basic materials, particularly metals, was completely inadequate to satisfy

all top priority demands; far too many instances had appeared when even the demands of highest-rated military orders could not be filled from the limited material supplies made available for these demands. The fact was assuming crisis proportions that shortages of vital raw materials, particularly the basic metals, and inequities in the distribution of these materials under the operation of individual allocation and preference rating systems, were throttling the efficient use of the American productive machine. Nothing short of carefully controlled over-all distribution of scarce materials seemed adequate to secure maximum utilization of facilities and full production of essential goods. But this necessitated considerable knowledge about both the demand for and the supply of scarce materials. Neither the tools for acquiring this information, nor those needed to use the information to balance the demand-supply equation existed when this country entered the war.

Notwithstanding the efforts of 1941, available facts about neither side of the demand-supply equation could provide a satisfactory basis for working a balance. Military requirements were unrealistic, inaccurate, and unbalanced within programs; they were more in the realm of "desires" than of "requirements." The OPM, and later the WPB, had progressed little further in developing comparable essential civilian requirements. Civilian programs were disorganized in the aggregate, in detail, and as related to military programs. Available estimates of both were far too high in totality, subject to rapid and unexpected shifts among programs within over-all totals, unadjusted for lead times and contingency allowances, and constantly in the process of changing specification and design. Uncertainties in the measurement of material supplies also created a stumbling block in equating the supply-demand balance. In some cases, calculations of both long-run and short-run estimates of material supply were so poor that later acquisition of accurate figures showed them to have missed by as much as 50 per cent. Overestimates and underestimates were of about equal frequency among various individual calculations. Thus, neither demands nor supplies in aggregate terms were accurate enough for intelligent policy and administrative action.

Inaccuracy, bias, and absence of facts as to both the demand and the supply sides of the raw material equation met head on in early 1942 with completely inept procedures for reconciliation of balance, even had there been sound requirement and supply figures. The WPB had not perfected within its own organization a systematic and effective review of the statistical, technical, and conceptual accuracy of even the

poor estimates of demand and supply which were available. There was no administrative machinery for equating demand and supply aggregates at policy levels. There were only the unsynchronized and crude procedures of M orders. Machinery was absent for scheduling raw materials and components through the fabricating chain with some semblance of coordination. As a result, each procurement agency, almost with impunity, heavily mortgaged critical materials, machine tools, labor, and other resources needed to protect its over-optimistic schedules. In the process no reference was made to competing claims of others. The WPB was helpless to reconcile and control these powerful influences, which worked towards imbalances throughout the nation's industrial productive machinery.

After more than two years' experience with wartime production control, facts simply were not available for the most fundamental type of policy decision. This situation was epitomized in the character of policy action taken by the WPB Requirements Committee which, in early 1942, knowingly issued its own over-all allocation directives with full consciousness that no mechanism existed by which it could determine whether its decisions were in full conformance with the over-all industrial situation; by which it could relate its actions to those taken in connection with individual materials or products, either by itself or by product administrators; and by which it could be sure that administration and execution of its actions was adequate for the fulfilment of its decisions. So explosive were pressures at the time that policy decisions had to be made with or without facts on which to base them.

These conditions led to the first concrete attempts made during the war period to obtain comprehensive information by which intelligent over-all metal-allocation policy decisions could be made. Throughout 1941, and continuing into 1942, substantial volumes of information were flowing to administrators of individual products as a result of the operation of P, M, and L orders. To coordinate information about these individual products and materials and to acquire comprehensive information by which war industrial activity could be measured and the demand-supply equation balanced, two industry-wide questionnaires were issued. The first was form WPB-732, which was designed to obtain comprehensive information about operations in the metal-working trades. The second was PD-275, which was designed to acquire plant-by-plant data concerning the use and requirements of critical metals. At the same time renewed pressures were exerted to obtain adequate requirements data from procurement agencies and to accumulate information on essential civilian demands within the WPB itself. These

measures stimulated a chain of events which eventually resulted in the accumulation of sufficient data by which policy and administrative actions were reasonably well supported and coordinated for the first time during the war period.

WPB-732. Form WPB-732 traced its ancestry back to the special plant reports collected by the Bureau of Labor Statistics for the NDAC. Upon the entrance of the United States into the war, it was recognized that a central war-control agency could ill afford to be without comprehensive knowledge relating to employment, machine and plant utilization, and other comparable data concerning all important industrial segments. By this time, too, it was clear that the main focus of the WPB control had to be centered upon the metal-fabricating trades. As a result, WPB-732 was inaugurated in March 1942 to obtain such facts from all important manufacturing plants.

WPB-732 was an operations-type statistical report submitted on a comprehensive scale. The original mailing list totaled about 15,000 plants in the metal-working trades but quickly rose to around 25,000. Towards the end of the war the list fell to approximately 9,000 of the largest plants, whose operations constituted about 85 per cent of total dollar shipments in metal fabricating industries. Information was requested concerning the extent to which plants had converted to war work, the extent to which plants were operating at capacity, products manufactured, unfilled orders, employment, machinery utilization, and explanations of failure to keep abreast of war contract delivery schedules.

Submitted on a monthly basis, its primary function was to produce a broad measure of the trends of war production and to reveal, for policy-making purposes, potential areas of production and distribution difficulty. So useful was the information collected from this form and so flexible was its method in adjusting to changing needs of management and to varying practices in industry, that it was destined to outlive the WPB. Throughout its existence, this form underwent constant revision to reflect changes in the preference-rating structure, the introduction of the Production Requirements Plan, and the Controlled Materials Plan; to include new types of data; and to adjust its informational requests to peculiarities of various industries. Throughout all these changes, however, the WPB maintained statistical threads in this questionnaire by which a continuous long-time historical series of comparable and related data could be maintained. This was the only comprehensive reporting instrument of the WPB to embody this feature.

The data collected were at all times valuable guides to policy on, and administration of, the armament production program. As a statistical rather than an application-type report, however, the figures were designed for policy rather than administration and did not constitute a vehicle for direct implementation of policy. But the figures revealed how policy and administrative action were working out in industry; served to detect potential problem areas; measured utilization of manpower and machinery in war work; and provided the basis for hundreds of special statistical reports upon which important decisions were made. Altogether, the data were used to throw light on a wide variety of problems.

PD-275. The introduction of form PD-275 in February 1942 firmly grappled with the materials-control problem facing the WPB. Experiences in late 1941 left no doubt that the prospective drain of an all-out war effort on the country's raw material supplies would become so great that only a coordinated materials-control plan would avoid industrial chaos. It was vital for the WPB to know over-all total requirements for basic materials and the supplies available to satisfy them. These requirements had to be classified according to major uses and time periods, and raw materials had to be scheduled through the industrial system on the basis of some sound order. A major contribution in the acquisition of the facts needed and the methods by which they could be used to exercise control was made by form PD-275.

PD-275 was essentially a modification of PD-25A currently being used under the (voluntary) Production Requirements Plan. Indeed, the form was so prepared that information requested on it was the same as the data asked for under the voluntary PRP. There were two major improvements, however. First, it was sent on a mandatory basis to 11,000 manufacturing establishments whose operations, it was calculated, would require approximately 90 per cent of the metals used in manufacturing. Second, a list of about 110 metal shapes and forms was devised from those under the PRP and preprinted on the form itself. The first change assured coverage; the second assured speed in tabulation. For each of the metals, the form requested data on inventories and consumption in the fourth quarter of 1941 and anticipated requirements in the second quarter of 1942. Manufacturers were asked to submit a separate form for each class of products manufactured if existing inventory and other records were such that separation was possible. This technique facilitated the reporting of data about each product.

The technical results of the survey were heartening. Summary tabulations provided the first reasonably accurate over-all measure of

metal consumption and requirements by manufacturing areas. The data revealed large lumps of critical metals lodging in excessive inventories, metal shipments on unrated orders, and metal demands carrying the highest ratings in excess of visible supplies. In other respects the summaries threw into sharp relief the fact that, if urgent war and essential civilian production was to be met, tighter and more efficient control over the flow of metals was necessary.

The WPB, however, was not quite ready for extended mandatory metals control on a comprehensive basis. A second PD-275 was therefore issued in April. This form was sent to 25,000 of the largest metal-using manufacturers and to shipyards, arsenals, railroads, and other consumers of metal. In layout, the form was similar to the previous issue, with one important modification. The form provided space in which manufacturers were required to report metal use and requirements for each class of products. Not only was the coverage of this form better than that of the first but the data obtained were more accurate and more readily tabulated.

The results of the tabulation of this survey reinforced those of the first questionnaire. Both made one fact stand out. The productive capacity of the United States was well beyond its ability to supply critical materials to industry, and, as a result, expansion of the production of the most vital military and civilian goods depended upon the immediate introduction of comprehensive coordinated control over the flow of basic metals. In addition, both forms, together with techniques built up through the operation of the (voluntary) Production Requirements Plan, revealed the administrative and statistical methods by which this could be accomplished.

The Production Requirements Plan. The WPB took the obvious step in the introduction of the (mandatory) Production Requirements Plan on June 10, 1942. Under this plan any plant, or division thereof, whose past or anticipated quarterly use or requirements of critical metals exceeded $5,000 was required to apply for rating assistance. The application form was PD-25A, a modification of the original PD-25A only in its incorporation of various technical lessons provided by PD-275. Essentially these were the preprinted metals stub and the direct relationship of metal use and requirements to individual final products. Exceptions to filing this application form were granted to United States or other government agencies (not including shipyards, arsenals, factories, etc., which were subject to the covering Regulation), transportation companies, public utilities, and so on. In practice, however, data were gathered by other means for these so-called

"exempt groups" so that a complete statement of over-all metal use became possible.

Each plant falling within the above requirements was required to submit data, on PD-25A, on the basis of the smallest breakdown of operations which existing raw material inventory records permitted. The calendar quarter was the time unit and for this time period manufacturers reported: (1) the dollar value of past and anticipated shipments for each final product, analyzed by preference ratings; (2) past consumption and future requirements for each of about 200 specified metal shapes and forms, for all operations of the plant; (3) past and anticipated inventory levels for each of the 200 metals; (4) past consumption and future requirements for each of 200 specified metal shapes and forms, for each final product; and (5) past consumption and future requirements for important components. Other information included rated orders, metal requirements for long periods in the future, and analysis of ultimate purchasers of products.

The first PD-25A, covering operations in the second quarter and requirements for the fourth quarter of 1942, was sent to approximately 32,000 plants. Successive quarterly operations extending through the second quarter of 1943 permitted a stabilization of the mailing list at about 30,000 plants or plant divisions. Of this number approximately 15,000 applications accounted for practically all metal consumption in manufacturing.

PD-25A fully covered industrial plant operations as they related to the consumption and use of metals. With this information, competition for basic metals in each plant could be appraised in terms of both mill supplies and the relative importance of end products. The entire structure of input-output relationships in American industry for the first time was silhouetted for study, analysis, and application to the problems of directing the flow of materials through industry. Further, mere addition of figures from each plant revealed reasonably accurate actual metal usage and potential requirements which, when related to calculated supplies and military and civilian programs, provided the means for the balanced allocation of materials in aggregates and in details by industries, plants, and products.

PD-25A approached more closely than any previous effort the sort of comprehensive reporting methodology needed in wartime. Tabulation of data on PD-25A put the Requirements Committee of the WPB in a position to pare total military and civilian requirements to match critical material supplies. When this was done, reduction in authorizations to buy and use metal was made by WPB administrative divisions

among various industry groups on a plant-by-plant basis. These decisions were transmitted to manufacturers by authorizations contained on their PD-25A application. PD-25A ,therefore, not only supported the highest policy decisions concerning metal allocations but also functioned as an instrument of administrative action.

Despite the potential usefulness of the PRP as an over-all control system, neither the preference-rating system nor individual allocation devices used by the WPB were eliminated by the adoption of the PRP. The effectiveness of the PRP approach to equate demand-supply imbalances and to produce order in the flow of materials was seriously impaired for these and other reasons noted below. The development of this new technique was accompanied by the adaptation and partial subordination of old methods rather than their complete abandonment. (This tendency toward superimposition of new controls on old ones was prevalent in the WPB's operations and accounts for much of the complexity and duplication which existed in the statistical structure.) In addition, no great progress was made in improving the techniques for calculating basic material supplies, in coming to grips with the problem of military and civilian requirements at the top procurement levels, or in controlling contract issuance.

Materials Accounting Reports. The effort to obtain a comprehensive system to produce facts to control metal flows brought with it a concept of basic material accountability. It was early recognized that accountability for action was an essential part of administrative order: it was one problem to obtain information upon which to determine basic policy and internal administrative actions to carry out policy, and quite another problem to be certain that internal operations and action at the production front were in conformance with policy. The latter assurance could come only in a system where current records of action were kept.

Toward the end of 1941 and early in 1942, efforts were made to control by accounting methods the mortgages placed on scarce metal supplies. They were not too successful. The adoption of the PRP, however, required the establishment of internal record-keeping systems by which current statements of metal authorizations could be reviewed. In no other way would it have been possible to determine whether the WPB administrators were authorizing materials within the limitations established by the Requirements Committee "pie cuts." As a result, comprehensive materials accounting was undertaken for the first time under the PRP.

The concept of materials accounting control growing out of this experience under the PRP constituted one of the major innovations in

the reporting system. So inherently necessary, sound, and successful were the general methods developed that control accounting grew to constitute an important part of many future control mechanisms within the WPB, in other wartime agencies, and in industry.

Vertical Versus Horizontal Allocation Reporting. The core of the production and distribution problem lay in the tying of materials needed by suppliers of parts and subassemblies to end products. In the fall of 1942 these problems assumed new importance, as fabricating facility shortages increased. Consideration of this problem brought a shift in thinking about basic over-all allocation control from comprehensive *versus* individual allocation systems (the PRP had settled that conflict) to comprehensive horizontal *versus* vertical allocation techniques. The concept grew more firmly that the horizontal material allotment approach of the PRP had to be discontinued in favor of a vertical material allotment approach. Thus, rather than allot materials directly to manufacturers at each fabrication stage, it was reasoned that metal flows could be tied tightly to end products if end-product producers allotted metals to their suppliers, the suppliers to their subsuppliers, and so on down the contractual chain to basic material producers.

Probably the most important shortcoming of the PRP as an allocation device (and the one from which most of its other deficiencies stemmed) was that it could not force a balancing of military and essential programs in relationship to limited metal supplies. The WPB through PD-25A was in a position to match total metal requirements, so far as they were known in industry, against total metal supply. Previously the WPB had relinquished its control over the contractual rights of procurement agencies and therefore was not in a position: (1) to force a balancing of military end programs within themselves; (2) to force a balancing of end programs within total material supplies; and (3) to force a tight distribution of metals and components in accordance with balanced programs. The PRP was forced to carry the burden for controlling the flow of metals and components in the absence of programming and scheduling, a burden it was not designed to carry.

In the absence of balanced military and essential civilian programs, the PRP and PD-25A could not effect the desired flow of materials through the industrial system nor could they govern the scheduling of component flows in the volume and time sequence required. When the total supply of tight materials was calculated by the WPB, requirements as submitted by industry on PD-25A could be matched against

it. This system produced facts on metal requirements of end programs for which contracts had been let at each level of manufacturing. These requirements could be matched against supply and allocation made accordingly. No mechanism existed, however, for forcing procurement agencies to pare contracts in conformance with this balancing of demand and supply. As a result, metal allocations made to a component producer, for example, often were not in conformance with the changing needs of end programs. In the absence of control over end programs of military agencies, serious difficulties arose with the PRP method of horizontal plant allocations whether such allocations were intelligently made or not.

These deficiencies of the PRP could be resolved, it was felt, by metal distribution on a vertical basis. With such a system the WPB would in effect create a bank balance for quantities of metal for each procurement agency. Those agencies in turn would write "checks" on their balances and distribute them to prime consumers. These proceeds would constitute a bank balance on the books of prime consumers from which they could make authorizations to their suppliers. Suppliers in turn would make extensions to sub-suppliers and so on. The net result would be, it was argued, that "bank withdrawals" could be kept within available balances and the distribution of materials would be tightly tied to and coordinated with production schedules.

After continuous discussion of alternative allocation proposals from early 1942 through the fall of that year, this approach to the problem of metal allocation was chosen. Among the more dominant schemes discussed were: a modified PRP, a "Warrant Plan," the "General Motors Plan," the "Material Allotment Plan," and the "Steel Quota Plan." Out of the exploratory discussions pertaining to these proposals and the critical materials situation there evolved the Controlled Materials Plan, the principal procedure of which was a vertical method of allocation. This plan was to constitute the only comprehensive metal allocation system in operation throughout the remainder of the war.

The Controlled Materials Plan. The Controlled Materials Plan was announced in November 1942, went into effect experimentally in April 1943 and fully in June 1943. The plan was based upon the theory that control over the production and distribution of a few basic materials —steel, copper, and aluminum—which were known as the "controlled materials," was sufficient to regulate a large part of industrial production. (For operational purposes these materials were broken down into 16 shapes and forms.) Control was exercised on the basis of two principles: (1) authorization of only as much production as available sup-

plies of basic metals would permit; and (2) establishing checks upon the use of these materials to maintain authorizations within limited supplies. To execute this control, four basic steps were followed: (1) determination of demands for materials; (2) determination of supplies of materials; (3) matching demand and supply; and (4) feeding material to scheduled production on a vertical-allotment distribution basis. At the apex of the pyramid up which demand-supply information flowed, and down which allotments were channeled, was the WPB Requirements Committee.

To the Requirements Committee, ultimate purchasers of all manufactured products containing controlled materials presented statements of their demands for those materials in terms of programs, physical metal quantities, and time periods. These purchasers, called Claimant Agencies, such as the Army, the Navy, the Maritime Commission, the Office of Lend-Lease Administration, and the Board of Economic Warfare, established so-called end programs designating equipment ultimately purchased for their account. Determination of material needed to fulfill these programs was facilitated by the acquisition from their prime contractors of "prototype" bills of materials (generally submitted on form CMP-1). Prime contractors in turn requested similar information from their suppliers, and so on down the contractual chain to the first fabricator of mill shapes. Thus, extending up through the contractual chain to the WPB Requirements Committee, detailed material requirements flowed. Controlled material supplies in various time periods were calculated by the WPB Controlled Materials Divisions. On the basis of this information, the Requirements Committee, in consultation with the materials Divisions and the Claimant Agencies determined how demand for various agencies and programs could be reduced to match supply. When this was accomplished the Requirements Committee made lump allotments against the supply to Claimant Agencies and their principal programs.

Upon receipt of allotments from the WPB, the Claimant Agencies distributed quantities among their own detailed programs. Authorizations for all detailed programs had to remain within total quantities received. Each agency, therefore, instead of competing with others through the issuance of preference ratings, worked toward a bank balance. If it wasted materials in one direction, it had less in another. Claimant Agencies then divided their allotments among their prime contractors, giving each enough controlled materials to carry on his part of the total program. Prime contractors, within the limits of their own "bank balances," passed along the portion of their allotments

needed by sub-contractors and sub-subcontractors to produce parts and subassemblies for their contracts. Thus, material distribution was passed down through the contractual chain. This simple procedure of following contractual channels tied metal demand to production schedules even though it did not reduce the dollar value of outstanding contracts.

The entire plan did not operate on vertical-allotment distribution. Components and shelf-items such as nuts, bolts, ball bearings, and electric motors were purchased by so many manufacturers for so many end uses that producers could not conveniently obtain controlled material allotments on the basis of vertical-allotment distribution. For these and other manufacturers of products such as pumps, gasoline engines and such wide-use components, who would have had trouble in receiving allotments by vertical distribution, the PRP method of horizontal allocation was adopted into the CMP. By this means, as under the PRP, these producers asked for and received allotments directly from the WPB.

To facilitate operation of the CMP, all finished products of manufacturers were classified into two groups, Class A and Class B. Class B products originally were intended to include only those subassemblies or miscellaneous or specialized items of equipment for which individual allotments could not practically be made from prime consumers down the contracting chain. This concept was modified, however, and many final products ultimately were defined as Class B products. All other products were Class A products.

The distinction between these two classifications was vital in CMP operations. All producers of Class A products were included in the vertical chain of allotment distribution, submitting their requirements (form CMP-4A) to their customers and receiving from their customers, in turn, metal allotments. Class B producers, on the other hand, were included in the horizontal-allotment distribution system, submitting directly to the WPB (form CMP-4B) their requirements for metals and, in turn, receiving from the WPB their needed allotments. As distinguished from the plant approach of the PRP, all CMP application and authorization documents were in terms of individual Class A or Class B products.

Insurance that demands were kept within metal supplies, once the WPB Requirements Committee had distributed allotments, was accomplished by an effective materials accounting system. Establishment of adequate accounting controls throughout the allotment system was based on the idea, born of experience, that effective control necessitated

procedures by which any overdrawing of accounts at any point in the distribution process could be prevented or detected. Application of this idea resulted in accounts of record showing for each program, for each material shape and form, for each quarterly period: authorizations, issuances, and balances. Such records were maintained in the WPB Industry Divisions, Claimant Agencies, and every industrial plant receiving allotments.

An effective check on the impact of metal authorizations at the basic-controlled-material-producer-level was accomplished by a system of periodic reports made by producers to the WPB Controlled Materials Divisions. On form CMP-8, producers reported shipments and unfilled orders. These reports were tabulated by the WPB and provided a direct measure of procurement pursuant to allotment by Claimant Agency and Industry Divisions, in terms of end programs for which original allotments were made by the WPB Requirements Committee.

The flow of end-use program information from Claimant Agencies through industry to metal mills was made on the basis of the passage of allotment program symbols. Originally, identification of allotments called for the passage of thirteen digits but the unworkability of the transmission of so many numbers resulted in a modification adopted before the Plan became operative, which provided only for identification of the Claimant Agency and the first digit of its program number.

Benefiting from the experiences of the PRP, various control systems, in effect in the WPB at the time the CMP was introduced were modified to conform with the latter. First, and of paramount importance, acquisition of controlled materials was only on the basis of direct allotment; the preference-rating system did not apply. Second, substantial modification or revocation was made among M, E, and L orders to avoid any interference or conflict with the operation of the CMP. As a result, some questionnaires established to operate individual allocation systems and other programs were abolished or substantially modified. The reporting and application forms of the CMP were given a dominant position which never had been extended to the PRP and its instruments.

At this stage of the war effort, inventories had ceased to be a serious problem. Unbalance between the demand for and supply of critical materials had flushed out most idle usable inventories. The CMP provided no automatic cleansing of idle inventories but rather exerted control by establishing a 60-day limitation on inventory accumulation. To assure compliance a plant report (form CMP-7) was issued covering operations in the third quarter of 1943. The form requested information

concerning plant shipments by products and for each CMP metal shape and form: actual use, receipts, and inventory. After one quarter of operation, the basic features of the form were incorporated into form WPB-732.

The CMP proved to be eminently successful in equating the demand-supply formula for controlled materials. Indeed the CMP, perhaps more than anything else, was a fruition of the concept that no WPB control policy over metals was adequate which did not devolve upon procurement agencies the responsibility for balancing their end programs and establishing methods by which these end programs could be translated into elemental common denominators of materials and processing time schedules. The CMP did force such a balancing and capitalized upon it by establishing suitable procedures in the WPB for acquiring accurate supply data and for equating demand and supply along realistic patterns. The CMP unquestionably firmly settled the problem of equating the demands for and the supplies of metals.

The plan was less successful in scheduling production. In operation, the CMP was primarily a materials-control system. It did not provide the machinery for balancing component production schedules in conformance with end programs. Claimant Agencies had the authority to spread out material allotments in conformance with their end-program requirements, but the flow of materials was not controlled in terms of time and volume through the industrial chain. As a result, component production for end programs was often far out of line with real requirements. Efforts to solve this problem were embodied in various regulations designed to supplement the operation of the CMP.

Scheduling Production. There emerged in full force in 1942 a group of problems relating to production scheduling. Although their emergence was detected early in the year, measures for their solution were not to be adopted until 1942 had almost ended. An analysis of four months of ordnance production, for example, indicated that this program was getting out of balance. The output of end items and the output of the complementary items essential to the use of the end items were not proceeding at the same rate. The scheduled production of components for artillery and aircraft ammunition was out of joint with the requirements for these components to meet production objectives for complete items. Many component schedules ran to two or three times requirements, and the schedules for two-thirds of the components called for production more than 15 per cent in excess of requirements. The result was that the ammunition program was using excessive quantities of

scarce materials. This and comparable situations demanded administrative processes by which the flow of critical materials would be controlled in accordance with scheduled uses, which should be limited to requirements for production objectives.

As the year 1942 moved on it became increasingly evident that, although total production was advancing, its rate of progress was leveling off. Solution to the problem lay not so much in basic material supplies, which had increased considerably during the year and had been matched with demand under the PRP, as in a system which defined points where materials were needed to effect a balance among end programs and which would direct critical materials to them.

The military services, which had undertaken the scheduling of production for military items, maintained little balance in schedules. Some schedules were over-optimistic, and others were conservative; there was little agreement as to lead factors, conversion periods, and provisions for the effect of changes in design and specifications. Moreover—and this was of crucial importance—there was no central review of the schedules for the purpose of reconciling competitive demands for common resources. And yet without such a review of schedules to align competing demands in relation to the maximum supply of common resources it was impossible to insure the development of realistic schedules. With the development of each new bottleneck in components, the record showed more clearly that inept scheduling had to be rectified if the nation was to squeeze the maximum production out of its resources.

In December 1942 a Production Executive Committee was established in the WPB with broad and inclusive powers to schedule components of all Claimant Agencies; to see that program schedules did not conflict, were in balance, and were consistent with the maximum productive possibilities of the national economy; and to ensure that schedules were in accord with the strategic requirements of the Joint Chiefs of Staff. A series of preliminary steps were taken to solve scheduling and related problems and in February 1943 General Scheduling Order M-293 was announced, to become operative on April 1, 1943.

The original M-293 applied to 35 classes of components classified into three categories of urgency. The WPB Industry Divisions were given full authority to control the shipment of these items. Information to exercise the necessary control was reported by manufacturers. Form PD-900 provided data on production capacity, orders, and order schedules for various periods. Forms PD-901 and PD-902 requested detailed delivery schedules. After the issuance of M-293, additional

WPB Industry Division scheduling orders appeared like dandelions in the spring and brought many new questionnaires with them. Since all these forms required details concerning scheduling of individual orders for individual items and customers, they more frequently than not were bulky and exceedingly burdensome to fill. Most of these orders were popular in industry, however, because by reason of them a manufacturer could freeze shipment schedules and go about his business without the need for constant adjustment and readjustment in schedules otherwise necessitated by the priorities system.

M-293 probably stimulated the flow of a larger volume of reports to the PWB than any other single regulation. After the novelty of component scheduling had worn off, most of the scheduling forms received by the WPB were filed away without review or any action whatsoever.

Reporting Trends in Other Areas. The preceding movements, viewed from an historical perspective, appear to be those of outstanding importance in the evolution of the reporting structure. Apart from them, however, there was constant evolutionary development, of a less spectacular character and with less fundamental impact upon industrial control, in hundreds of individual products and materials. In each instance in which the demand-supply equation for a particular product was so unbalanced as to impede production, special reporting requirements were instituted to provide facts for intelligent administration. When the problem ceased to exist, reporting was generally discontinued. The often unpredictable impact of war upon the thousands of products needed for its prosecution created a substantial ebb and flow of control problems which in turn was reflected in the rise an discontinuance of thousands of individual questionnaires.

The use of control procedures and reporting methods adopted to support them often paralleled and leaned heavily upon the experiences described above. Thus, for example, the methods of the Lumber Control Plan and the Tire Allotment Plan were essentially those developed along PRP lines.

By the middle of 1944 the basic WPB reporting structure was created. Thereafter, its movements were essentially caused by modifications of existing reports rather than by the introduction of revolutionary new methods. With the approach of VE-Day, and later VJ-Day, strenuous efforts were made to simplify the reporting structure by eliminating from it all questionnaires of little or doubtful use. The result was that in the period immediately preceding VJ-Day the WPB operated with fewer questionnaires than in any previous period after the entrance of the United States into active warfare.

ON POOLING DATA

FREDERICK MOSTELLER
Harvard University

On the basis of samples from normally distributed variates X_1 and X_2, how should one estimate the mean of X_1? Should one pool or not pool the means on the basis of a significance test, or should one pool continuously, or not at all? It turns out that if the difference between the true means can be thought of as normally distributed from sample to sample, pooling with unequal weights is preferable.

INTRODUCTION

THE PURPOSE OF this article is to discuss a very simple problem concerning the pooling of data. If we have a sample of n measurements from population A and a sample of n measurements from population B, where A and B are similar, how shall we estimate the mean of population A? Shall we merely take the mean of the sample from A, or shall we attempt to combine the means of the two samples in the hope of improving our estimate of A in some average sense?

A variation of this problem is frequently met in the analysis of variance where various estimates of error are sometimes pooled after an initial significance test has been made. This case has been treated in some detail by T. A. Bancroft. The problem of pooling means does not seem to have been treated. The principal reason for discussing the pooling of means is that the notions are much more easily understood than the corresponding question of pooling sums of squares. Considerable insight into the general pooling problem can be gained by an examination of the more elementary problem. The problem of the pooling of means can be discussed in an elementary class in statistics. In addition, there are occasions when the question of the implications and the results of pooling means arises in practice.

If we have two (or more) similar measures of an individual's performance, is it preferable to take the later measure as an index of his ability, or would it be preferable to average the measures? If a producer sells us two very similar products made of the same materials on the same machines by the same operators, shall we estimate his performance on one of the products by the per cent defective observed on that product, or can we get a better picture by combining the results of samplings on both products? This question may be of considerable interest if the testing happens to be destructive and expensive. These and similar

questions are all concerned with the pooling of means. We shall, of course, idealize the problem considerably.

Let us suppose we have two kinds of "12 inch" rulers, which for convenience we designate as "red" and "black." We assume that both kinds of rulers have lengths which are approximately normally and independently distributed with equal known variances σ^2, but unknown means, u_r and u_b, where the subscripts indicate the color of the ruler. We take random samples of n red rulers and n black rulers. Let $\bar{x}_r$, $\bar{x}_b$ be the sample means, then for $k>0$ if

$$(1) \qquad |t| = \frac{|\bar{x}_r - \bar{x}_b|}{\sigma}\sqrt{\frac{n}{2}} < k$$

we accept the null hypothesis that $u_r = u_b$, at significance level

$$\alpha = 1 - \frac{1}{\sqrt{2\pi}}\int_{-k}^{k} e^{-\frac{1}{2}t^2}dt.$$

If the inequality in (1) does not hold we reject the null hypothesis in favor of the alternative $u_r \neq u_b$.

Once the significance test has been made, the following question sometimes arises. On the basis of the samples, how should one estimate the mean length of the red rulers?

When the null hypothesis is rejected, many feel that the "best" estimate of u_r is $\bar{x}_r$. We know that the variance of $\bar{x}_r$ is

$$(2) \qquad \sigma^2(\bar{x}_r) = \frac{\sigma^2}{n},$$

if we always use just the sample of n red rulers to estimate u_r. But when the null hypothesis is accepted some people say: "Now that we have accepted the null hypothesis, $u_r = u_b$, the 'best' estimate of u_r is the grand average of the lengths of the $2n$ rulers $(\bar{x}_r + \bar{x}_b)/2$." People with a little different philosophy argue: "No matter how many significance tests you make you can never prove the null hypothesis. You should forget all about the significance test. The 'best' estimate of u_r is $\bar{x}_r$, the mean of the n red rulers." Still others say: "This is a problem of estimation, not one of tests of significance. One should merely inquire what weights to assign to $\bar{x}_r$ and $\bar{x}_b$." No doubt still other people hold still different views.

It may well be that the criterion selected as a significance test and the criterion selected for deciding to pool or not to pool are two different things. For the purpose at hand we shall work with a model for pooling

which uses the same test as that given in equation (1). However one may still wish to use a value of k for a significance test which is different from that for the pooling criterion. That is, if one were using a .01 level for a significance test one might require a .20 level, say, before pooling with equal weights.

It should not be thought that the above discussion implies any ambiguity about the meaning of a significance test. Rather, the ambiguity, if it exists, has to do with the use of the usual concept of test of significance as a basis for estimation and action. Of course, the mathematical principles laid down for the use of a significance test and the actual procedure followed often differ. For example, in theory one sets a significance level in advance, one makes a test of significance, and one accepts or rejects the null hypothesis at the significance level previously chosen.

Actual practice usually differs from the procedure outlined above. The person working with the data has no fixed criterion in mind, but rather a vague feeling that the .05 level of significance is pretty widely used, that the .01 level is somewhat more stringent, and that the .001 level is something an experimenter's results do not exceed too often. A standard calculation procedure is now performed. Now the experimenter tends to report the percentage of times his result would have been exceeded had the null hypothesis been true. The words "highly significant" are usually tossed about in connection with results beyond the .001 level, and special remarks are included about results which just do not or barely do exceed the .05 level.

One might argue correctly that those who practice the procedures followed above are not performing significance tests at all, but that they are using the standard procedure to help describe the data and the results of the analysis. Nevertheless, a very large proportion of those who analyze data follow the practice of not setting a significance level in advance (or at most a fuzzy one), and at the same time refer to their procedure as a significance test.

The purpose of the present discussion of theory and practice in connection with significance tests is not meant as a criticism of anyone's procedure. It is presented, because it helps indicate how arguments can arise about the interpretation of significance tests.

PRECISION OF ESTIMATION

First we need a preliminary agreement upon an objective way of measuring which of two estimates is the better. If it is insisted that the estimate of u_r must always be unbiased, proponents of pooling the two

samples have already lost their case (so poolers should not be among those who insist on unbiased estimates). However a more temperate view might be that the mean-square deviation D^2 from the true parameter value u_r is a reasonable measure of the goodness of the estimate. We shall use this criterion, although others are equally valid. This criterion has some computational advantages for the models we will construct.

If we always pool the two samples with equal weights, the mean-square deviation of the pooled estimate of u_r, $\bar{x}=(\bar{x}_r+\bar{x}_b)/2$, is

$$D^2(\bar{x}) = \frac{\sigma^2}{2n} + \frac{d^2}{4}, \tag{3}$$

where $d=u_r-u_b$, the difference between the means of the red and black rulers.

On the basis of the mean-square deviation criterion, those who recommend constantly pooling with equal weights have the advantage when

$$\frac{n}{2} d^2 < \sigma^2,$$

while those who recommend using only the measurements from the n red rulers have the advantage when

$$\frac{n}{2} d^2 > \sigma^2.$$

If d^2 is as great as $2\sigma^2$, there is never any advantage in pooling. Furthermore, for any fixed σ and d, d not zero, we can always take samples sufficiently large that there will be no advantage in pooling.

ESTIMATION WITH FIXED DIFFERENCE

We have not done complete justice to those recommending pooling. They point out that they only pool when their criterion suggests that they should. Consequently, we must evaluate the expected value of

$$[\tfrac{1}{2}(\bar{x}_r + \bar{x}_b) - u_r]^2$$

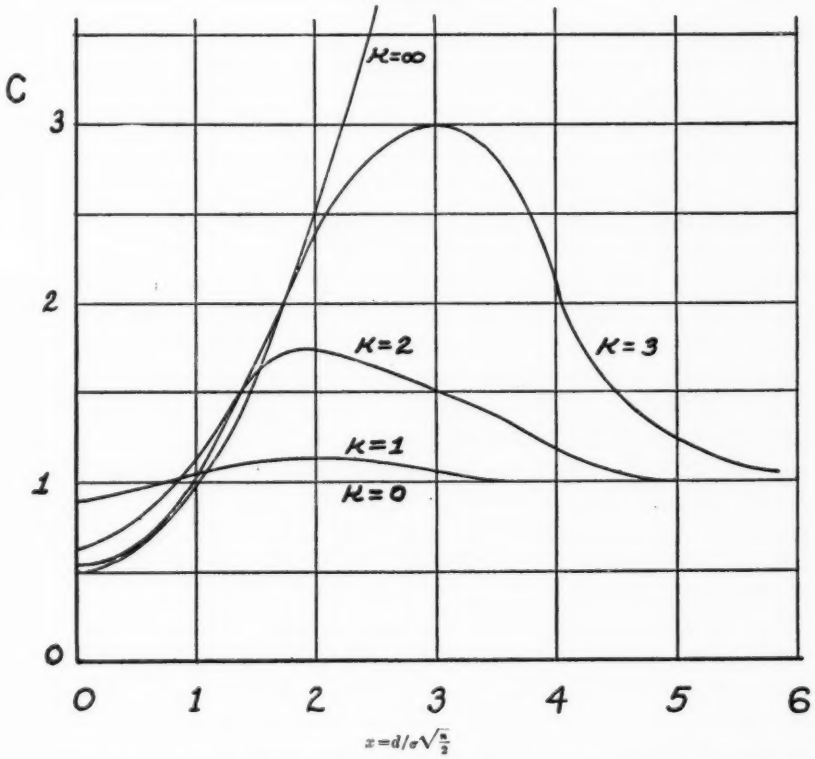
over the acceptance region, plus the expected value of $(\bar{x}_r-u_r)^2$ over the rejection region. When this is done we can divide the mean-square deviation by σ^2/n, the variance when one never pools, and thus get a disadvantage coefficient. Essentially we are taking the variance of the estimate $\bar{x}_r$ as the standard.

The integrations are easy to perform, and the disadvantage coefficient turns out to be

$$C = \frac{1}{2}\left\{1 + x^2\int_{-k-x}^{k-x} N(y)dy + (k-x)N(k-x) + (k+x)N(k+x) + \int_{k-x}^{\infty} N(y)dy + \int_{-\infty}^{-k-x} N(y)dy\right\}. \tag{4}$$

In equation (4), $x = d/\sigma\sqrt{n/2}$, the distance between the true means measured in terms of the standard deviation of the difference of the sample means; k is the critical ratio indicated in equation (1); and $N(U)$ is the normal distribution $e^{-1/2U^2}/\sqrt{2\pi}$.

FIGURE 1



THE DISADVANTAGE COEFFICIENT C AS A FUNCTION OF $x = d\sqrt{n}/\sigma\sqrt{2}$ FOR $k = 0, 1, 2, 3, \infty$, WHEN d IS FIXED. IF $|t| < k$, $\hat{x} = (\bar{x}_r + \bar{x}_b)/2$ IS USED TO ESTIMATE u_r, WHILE IF $|t| \geq k$ THE ESTIMATE $\bar{x}_r$ IS USED.

The behavior of C is more easily understood if one examines the graph shown in Figure 1. We only show positive differences, because the figure is symmetrical about $x=0$. Now providing the difference is very small we can get advantages from pooling up to 10 per cent when $k=1$, up to about 35 per cent when $k=2$, up to 47 per cent when $k=3$, and up to 50 per cent when $k=\infty$. When $k=0$ we never pool—that is, we never expect to find $\bar{x}_r=\bar{x}_b$, exactly. When $k=\infty$, we always pool, that is we set our criterion so large that we invariably pool.

The disadvantage of pooling occurs when x gets large. If we always pool, the mean-square deviation can be so large that the disadvantage coefficient tends to infinity. If we have any criterion which stops us from pooling, it will happen that when the difference between means gets too large we will not pool. Consequently for any finite value of k, as x increases, the disadvantage coefficient will rise to a maximum, and then return asymptotically to $C=1$. It will be noted that when $k=3$ it can happen that the mean-square deviation is about three times the variance we get when we never pool. Similarly for $k=2$, C can grow as large as 1.75, while for $k=1$ the maximum for C is about 1.12.

Diagrams such as Figure 1 suggest that if one has any additional information about d, then something might be gained by pooling or not depending on the indication of the figure. For example, if one were confident that $|d| \leqq 1$, $\sigma=30$, $n=50$, then $x \leqq .167$. In this case one should always pool.

There are examples where the distribution of the true mean can reasonably be thought to be approximately normally distributed. If we examine the day to day performance of an individual on certain psychophysical tests after the initial training period we find that the distribution of his test-to-test means can be approximated by a normal distribution even though his test-to-test variation cannot be explained by random sampling errors from a single population. Also, if we examine the distribution of mean *educational age* of 6th grade classes in a certain area, we will find these means to be approximately normally distributed but with variance too large to be explained by random sampling from the same population.

It is not the intention of the author to argue that there are large numbers of examples of normally distributed means, but rather that there are many examples of cases where the parameter can be thought of as having a distribution of some sort. In such cases there is a possibility of improving an estimate of u_r if it is known that two samples (or more) are drawn one from a population with mean u_r, another from

a population with mean u_b, where both u_r and u_b are samples from a parent population of true means.

ESTIMATION WHEN DIFFERENCE IS DISTRIBUTED

We have seen how the disadvantage coefficient C behaves when the difference d between the means is fixed. In a series of experiments, or a lifetime of estimation, we might prefer to think of d as varying. If we knew something about the frequency with which d takes on different values we might be able to take this into account. Of course this is the fundamental Bayes Theorem difficulty. However if we are willing to consider the problem from the point of view of a long series of similar experiments, we can construct some instructive models.

For example, we might be willing to assume that d is normally distributed with zero mean and variance $a^2\sigma^2$, for a series of experiments of a certain type for which we would take samples of the same size n. It is not unreasonable to assume that d is symmetrically distributed about zero, because the choice of the order $u_r - u_b$ or $u_b - u_r$ is arbitrary. The assumption of normality is a more difficult case to argue. It will not be attempted. Instead it is suggested that it would be useful to have a number of additional models of various kinds worked out for problems of this nature.

STANDARD ERROR OF THE ESTIMATE

It needs to be emphasized that in some cases one is anxious to have an accurate estimate and is willing to sacrifice knowledge of its accuracy, while in other cases it is very important to have a "reasonably" unbiased estimate of the standard error of the estimate. If one never pools, one can use σ^2/n as the standard error as usual. In cases where we always pool the standard error of the estimate will be $\sigma^2/2n$, even though the estimate may be biased considerably. When we use a "sometimes pool" procedure the situation is much more complicated. It would, of course, be inaccurate to assign the standard error the value σ^2/n, when we do not pool, and $\sigma^2/2n$ when we do. Instead we must think of the distribution of the estimate of the mean and get its standard error for repeated sampling. The standard error will depend on the significance level and on the true difference between the means. We may underestimate the standard error if we use either of the above formulas.

If it is assumed that d is normally distributed, we may expect the poolers to continue to pool when $|t| < k$, and not to pool when $|t| \geqq k$, making their estimates in the same way as described above.

Again we will compute the disadvantage coefficient C. This time the integrations are more tedious, but we come out with

$$C = 1 + \frac{y-2}{2}\left\{-k\sqrt{\frac{2}{y+2}}N\left(k\sqrt{\frac{2}{y+2}}\right) + \int_0^{k\sqrt{2/y+2}} N(v)dv\right\}, \tag{5}$$

where $y=na^2$. It will be remembered that n is the sample size, $a^2\sigma^2$ is the variance of d, and k is the criterion for pooling or not pooling. As before $N(U)$ is the normal distribution with zero mean and unit variance.

An examination of equation (5) shows that when $k=0$, $C=1$, which is what we expect if we never pool. Further, when $k=\infty$ we always pool, and the limit $C=(2+na^2)/4$, which can easily be checked to be the expected value of $D^2(\bar{x})/\sigma^2(\bar{x}_r)$ when d has the distribution we have assigned it.

It can be shown that when y is large C tends to unity asymptotically from above like $1+k^3\sqrt{2}/3\sqrt{2\pi y}(1+5/y)$. This means once again that if a pooler almost always gets large discrepancies in the t-test, he will only pool occasionally, and the effect will be essentially as if he did not pool at all.

Figure 2 shows more clearly how C behaves as $\sqrt{y}=a\sqrt{n}$ goes from 0 to 6. We see that all the curves for $0<k<\infty$ are contained between the curve $C=(2+y)/4$ for $k=\infty$ and the straight line $C=1$ for $k=0$. It can be verified from equation (5) that when $\sqrt{y}=\sqrt{2}$, $C=1$.

With the present model we note that for small values of $na^2=y$ there are advantages up to 50 per cent to be gained by pooling. For $na^2=y>2$ some loss in precision will occur. However, with the present model the losses do not go as high as when we dealt with a fixed value of d (Fig. 1), providing $k<\infty$.

For convenience, we have graphed C against $\sqrt{y}$, for $0\leqq\sqrt{y}\leqq 6$. For $k=3$ a very flat maximum occurs at about $\sqrt{y}=4.5$, and $C=1.7$; for $k=2$ the maximum occurs at about $\sqrt{y}=4$ and $C=1.2$; while for $k=1$, the maximum occurs at about $\sqrt{y}=3$, and $C=1.04$.

We compare the maximum C's for the two models discussed in Table I.

Of course, even if one accepts the present model, one still has to make a decision between pooling and not pooling. We do not usually enter into an important experiment with no ideas at all about the order of magnitude of the quantities we will find.

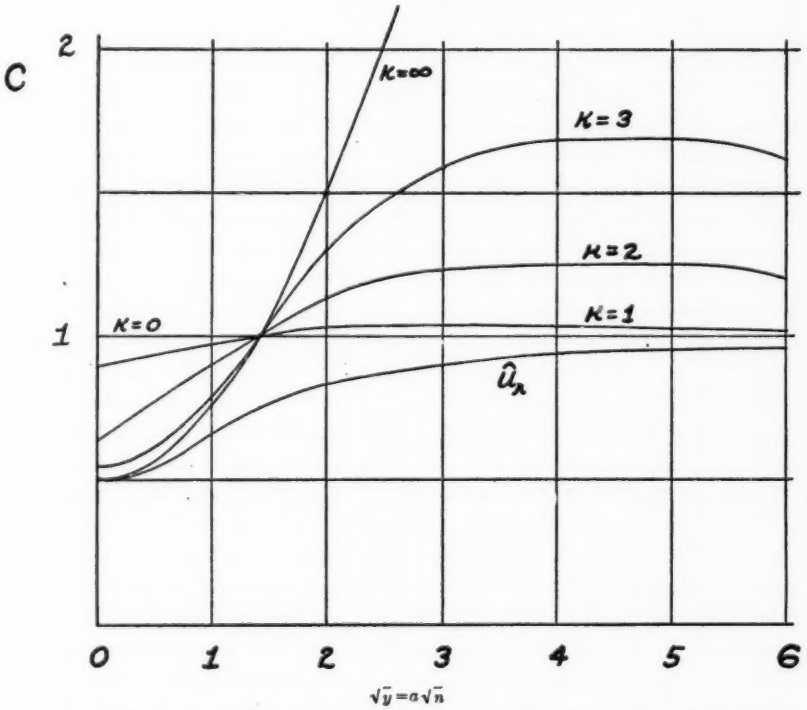
TABLE I

MAXIMUM DISADVANTAGE COEFFICIENT C IN POOLING FOR TWO CASES: 1) FIXED DIFFERENCE BETWEEN MEANS d; 2) d IS NORMALLY DISTRIBUTED. THE CRITERION FOR POOLING OR NOT POOLING IS k

k	d fixed	d normal
0	1.00	1.00
1	1.12	1.04
2	1.75	1.2
3	3.0	1.7

For example, suppose we believe that the standard deviation of the difference between the mean lengths of successive pairs of shipments of red and black rulers is about .001 inch, that the standard deviation of red and black rulers is about .010 inch. If we take a sample of 50

FIGURE 2



THE DISADVANTAGE COEFFICIENT C AS A FUNCTION OF $\sqrt{\bar{y}}=a\sqrt{n}$ FOR $k=0$, 1, 2, 3, ∞, WHEN d IS NORMALLY DISTRIBUTED WITH ZERO MEAN AND UNIT VARIANCE. IF $|t|<k$, $\hat{z}=(\bar{z}_r+\bar{z}_b)/2$ IS USED TO ESTIMATE u_r, WHILE IF $|t|\geqq k$, THE ESTIMATE $\bar{z}_r$ IS USED. THE LOWEST CURVE, $\hat{u}_r$, IS BASED ON THE MAXIMUM LIKELIHOOD ESTIMATE $\hat{u}_r=(\bar{z}_r(1+v)+\bar{z}_b)/(2+v)$.

rulers of each color, then $a = .001/.010 = 0.1$, and $a\sqrt{n} = .707$. So no matter whether we are using $k = 1$, 2, or 3 as our criterion, Figure 2 indicates that the advantage is "probably" with the poolers. The word "probably" is used because the quantity .001 inch given above is really a guess on the part of the experimenter. There is nothing reprehensible about intelligent guessing. The most successful experimenters are those who combine a good knowledge of their subject matter and of experimental design with ability to make judicious guesses. The efficiency of experiments can almost always be improved by good guessing.

On the basis of this model, therefore, if there is some possibility of making intelligent guesses about a and σ, the author would decide between pooling and not pooling on the basis of C and the sample. In the absence of such guesses the author feels that between those who never pool, and those who require a two-sigma agreement ($|t| < 2$) or better before pooling, there is little to choose. It does not seem likely that the latter can be injured badly on the average, and they may gain considerably.

On the basis of the present investigation the author's principal reason for disliking the use of a k as liberal as 3 comes from the disadvantage factors when d is regarded as fixed. It is always possible for an experimenter to encounter a series of experiments where d does not behave as it is assumed to do in the variable model, and in that case one may very well be multiplying the no-pooling precision by a factor of 3.

Of course, if the observations are cheap and easy to get, the problem scarcely arises; one merely takes a larger sample of red rulers.

MAXIMUM LIKELIHOOD POOLING

We have gone to a good deal of trouble to inquire about what happens when the decision to pool or not to pool is either made a priori, or made on the basis of a criterion which is equivalent to a significance test. We have kept to this line because we were interested in this question, and because this is a method often used in pooling.

An important model which might be mentioned however is the maximum likelihood method. If we consider the case of d fixed, the maximum likelihood estimate is $\bar{x}_r$, and $C = 1$, as we have noted. But if we assume as in the previous model that d is normally distributed, the maximum likelihood estimate suggests that we always pool, but assign varying weights to $\bar{x}_r$ and $\bar{x}_b$. The maximum likelihood estimate of u_r is just

$$\hat{u}_r = \frac{\bar{x}_r(1 + na^2) + \bar{x}_b}{2 + na^2}. \tag{6}$$

If $na^2=0$, that is $d=u_r-u_b=0$, the pooled estimate $(\bar{x}_r+\bar{x}_b)/2=\bar{x}$ is the maximum likelihood estimate. If na^2 is extremely large compared to $\bar{x}_r$, $\bar{x}_b$, and 2, then the maximum likelihood estimate reduces to $\bar{x}_r$.

The mean-square deviation of $\hat{u}_r$ from u_r, using the model with d normally distributed gives

$$D^2(\hat{u}_r) = \frac{\sigma^2(1 + na^2)}{n(2 + na^2)} . \tag{7}$$

Here we see that as na^2 goes from zero to infinity the pooled estimate has mean-square deviation running from $\sigma^2/2n$ to σ^2/n, that is from the variance for $2n$ observations to the variance for n observations. The lowest curve (labelled $\hat{u}_r$) of Figure 2 is a graph of $D^2(\hat{u}_r)$.

Equation (7) is also the asymptotic variance of $\hat{u}_r$ given by maximum likelihood methods when the true value of a^2 is unknown, but to be estimated from $\bar{x}_r$ and $\bar{x}_b$. It is not clear however exactly what estimate of a^2 should be used for small samples. However, the estimate is approximately $(\bar{x}_r-\bar{x}_b)^2/\sigma^2$. If this value is used for a^2 in equation (6) it is clear that when the difference between the sample means is large $\bar{x}_r$ gets most of the weight, while if it is small the weights are more equally balanced between the sample means.

The curve for $\hat{u}_r$ in Figure 2 shows that the disadvantage factor for the maximum likelihood method is always less than unity. This implies that if one has reason to believe that d is normally distributed the maximum likelihood solution is by far the most profitable among the ones discussed. The implication is that at least for this situation pooling with unequal weights is usually the best policy. The all-or-none criterion method cannot compare with the estimate of equation (6).

This result is rather important because it backs up our intuitive feeling that we should use all the information at hand in making our estimates.

CONCLUSION

We have presented and discussed several ways of pooling data from two samples to estimate the mean of the population of one of them. Perhaps the most important point of the discussion is that for many kinds of estimation, not only of means, we need more models to study. These models help us understand what is going on when we pool data from several samples to make a single estimate. Furthermore they may assist us in deciding which of several possible methods of combining data may be preferable.

The author is indebted to J. Wolfowitz for calling his attention to an error in an early draft of this paper.

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USE OF VARIANCE COMPONENTS IN THE ANALYSES OF HOG PRICES IN TWO MARKETS

R. L. ANDERSON
Institute of Statistics, North Carolina State College

Corrections to article published in Volume 42, No. 240 December 1947

Page 618, Table 4: The variance components for the *days* mean square should include $24\sigma^2_{dy}$.

Page 624, line 2 should read: $\doteq V_{dmy}+V_{myc}-V_{dmyc}$

Page 624, line 4 above the table should read:

$$M: \quad V_{mc} + V_{dmy} - V_{dmyc}.$$

Page 625 in the Table for F', last line, should read: 3.34*.

Page 360, line 6 from the bottom should read: $a_j = \pm 1$.

ON ESTIMATING PRECISION OF MEASURING INSTRUMENTS AND PRODUCT VARIABILITY

FRANK E. GRUBBS

Ballistic Research Laboratories, Aberdeen Proving Ground, Md.

A measurement or observed value is discussed as the sum of two components—one the absolute value of the characteristic measured and the other an error of measurement. The variation in absolute values of the characteristic or items measured is termed product variability, whereas the variation in errors of measurement of an instrument is called the precision or reproducibility of measurement. Techniques are given for separating and estimating product variability and precision of measurement. Comparisons of the various techniques are also discussed for cases involving two or more instruments.

1. INTRODUCTION

A MEASUREMENT consists of the sum of two components, one component being the true or absolute value of the item or characteristic measured and the other an error of measurement. In making measurements, the aim is to obtain as nearly as practicable the true or absolute value of every characteristic measured. This is accomplished either by attempting substantially to eliminate errors of measurement or by using an instrument for which the errors of measurement are known to a sufficient or estimable degree. In particular, if the errors of measurement are for all practical purposes constant then rather accurate knowledge of the true values of the items measured can be ascertained when the size of the constant error can be calibrated by reference to a standard. On the other hand, if the errors of measurement vary appreciably from one observation to another in a series of observations, then the accuracy and usefulness of the reported results will depend largely on available knowledge concerning the relation between the errors of measurement and the true values of the characteristics or items measured. Thus, as the general demand for closer tolerances increases it becomes important to investigate methods for studying the relation between the two components of measurement in order to insure that the instrumentation involved meets its purpose adequately.

The purpose of the present investigation, therefore, is to present a statistical study of methods for estimating and comparing product

variations and errors of measurement. The methods discussed are quite general and should apply to most instrumentation set-ups which can be employed as outlined below. The problem itself arose at Aberdeen Proving Ground in connection with investigations of the adequacy of electric clocks in measuring burning times of powder train fuzes, in measurements of velocity with two or more chronographs, and in measures of rotating band clearance, to mention some.[1]

2. MEASUREMENTS WITH A SINGLE INSTRUMENT

Suppose a single instrument is used to measure some characteristic of n different items. The results of measurement may be represented symbolically as follows:

$$\begin{aligned} &x_1 + e_1 \\ &x_2 + e_2 \\ &\vdots \\ &x_i + e_i \\ &\vdots \\ &x_n + e_n \end{aligned}$$

where $x_1, x_2, \cdots x_1, \cdots x_n$ are the true values of the characteristics or the items measured and the quantities, $e_1, e_2, \cdots e_i, \cdots e_n$ are errors of measurement. Generally speaking, $x_1, x_2, \cdots x_i, \cdots x_n$ are all different since there is variability in the items or product measured. Also, the quantities e_1, e_2, etc. will usually vary because a different error in measurement may be made on each item. It is to be noted here that in the single measurement or observed value, x_i+e_i, the error of measurement, e_i, is inseparable from the true value of the characteristic, x_i. That is, for example, a single micrometer measurement may be, say, .5635 inch and it is not known precisely what part of .5635 inch is the error of measurement, which in this case arises from the combination consisting of the operator and the micrometer. If all the errors of measurement e_1, e_2, etc. were zero it would be said that the instrument is accurate or perfectly calibrated for each measurement (or that the measurements are absolute) and also that the utmost in precision or reproducibility has been attained. Precision here is defined as a measure of the variation in the errors of measurement,

[1] Somewhat similar problems, namely those of estimating components of variance, are treated in Snedecor's book *Statistical Methods*, Iowa State College Press, under the index heading "Variance, Components of" and in a paper by H. E. Daniels "The Estimation of Components of Variance," *Journal of the Royal Statistical Society*, Supp., Vol. VI, No. 2, 1939, pp. 186–197.

e_1, e_2, etc. If the quantities e_1, e_2, etc. vary considerably it may be said that the measuring instrument lacks precision. On the other hand, if all of the errors of measurement are equal, i.e. if $e_1=e_2=\cdots=e_i=\cdots e_n$, our instrument possesses the utmost in *precision* because the same constant error in measurement is made each time. However, such an instrument does not give absolute measurements unless corrections are made for the constant error of measurement. It is seen, therefore, that accurate knowledge of the true values or characteristics, x_1, x_2, x_3, etc. cannot be obtained unless something is known about the errors of measurement, e_1, e_2, etc.

If the measured or observed values x_1+e_1, x_2+e_2, etc. are averaged, one obtains

$$\frac{\sum_{i=1}^{n}(x_i+e_i)}{n}=\bar{x}+\bar{e}$$

where $\bar{x}$ is the average value of the characteristic measured and $\bar{e}$ is the average error of measurement. (As in the case of x_i+e_i above, the average observed value, $\bar{x}+\bar{e}$, consists of inseparable components also.) If actually, $\bar{e}=0$, then it may be said that the errors of measurement average out to zero and that the average observed value would be "accurate"; but on the other hand, the measurements may not be *precise* since the individual errors of measurement may vary considerably from one observation to another.

In order to determine the precision or reproducibility of measurement, the amount of variation in the quantities e_1, e_2, etc. should be examined, especially as compared to the variation in x_1, x_2, $\cdots x_n$. Now, the variance of the observed measurements is given by

$$\frac{1}{n-1}\sum_{i=1}^{n}\{(x_i+e_i)-(\bar{x}+\bar{e})\}^2$$

$$=\frac{1}{n-1}\sum_{i=1}^{n}\{(x_i-\bar{x})+(e_i-\bar{e})\}^2$$

$$=\frac{1}{n-1}\sum_{i=1}^{n}(x_i-\bar{x})^2+\frac{2}{n-1}\sum_{i=1}^{n}(x_i+\bar{x})(e_i-\bar{e})$$

$$+\frac{1}{n-1}\sum_{i=1}^{n}(e_1-\bar{e})^2.$$

The quantity

$$s_x^2 = \frac{1}{n-1}\sum_{i=1}^{n}(x_i - \bar{x})^2,$$

which is based on a sample of n measurements, gives an unbiased estimate ($n-1$ degrees of freedom used) of the true variation or variance in the characteristic x of a larger category from which the random sample of n observations is taken. In a like manner,

$$s_e^2 = \frac{1}{n-1}\sum_{i=1}^{n}(e_i - \bar{e})^2$$

is a measure of the variation in the errors of measurement and gives an unbiased estimate of the variance in errors of the measuring instrument. The quantity,

$$s_{xe} = \frac{1}{n-1}\sum_{i=1}^{n}(x_i - \bar{x})(e_i - \bar{e}),$$

known as a co-variance, gives a measure of the relation between the values of the characteristics measured and the errors of measurement. In this discussion, it will be assumed that over a somewhat limited range in values of the characteristic, x, no correlation exists between the values of the characteristic measured and the errors of measurement, and consequently that the mean or expected value of the covariance, s_{xe}, will be zero (see below). It may be true that errors of measurement in some cases increase with increasing magnitude of the characteristic measured; however, in many problems the x and e are sufficiently independent to insure that limited variations in x are not reflected in the errors of measurement.

If s_e^2 is small (on the average), relatively speaking, then it may be said that the measuring instrument is precise; on the other hand, the larger s_e^2, the more imprecise are the measurements. If s_x^2 is small, there is little variation in the characteristic measured: otherwise the variability in the characteristic measured or product is not small. The efficiency with which the absolute values $x_1, x_2, \cdots, x_n$ are measured depends on (1) the relative size of s_e^2 and s_x^2, the two components it is desired to estimate and (2) proper calibration of the measuring instrument. An important point of interest here is that in using a single measuring instrument on a group of items, the variance of the observations consists of the sum of *inseparable* components—one the variance

in the items measured and the other the variance in the errors of measurement.

With a single measuring instrument, therefore, it apparently is not possible (1) to separate an individual error of measurement, e_i, from the value of the characteristic, x_i, (2) to separate the *average* error of measurement, $\bar{e}$, from $\bar{x}$, the average value of the characteristic measured and (3) to separate the variance in errors of measurement from the variance in the characteristics measured in order that the relative magnitude of these components could be studied. It is true that by measuring the same characteristic over and over again without bias or if there is no variation in the product (i.e. if $x_1 = x_2 = \cdots = x_n$), then s_e^2 could be obtained directly. Also, $\bar{e}$ could be determined if the value of x were known or if the instrument itself were calibrated accurately against a standard.

The usefulness of a measuring instrument depends therefore on knowledge concerning the errors of measurement and a factor of primary importance is the relative order of magnitude of s_x^2 and s_e^2. Thus, methods are sought for separating the components, s_x^2 and s_e^2. The actual separation of s_x^2 and s_e^2 requires the use of at least two instruments as pointed out below. First, however, certain definitions and assumptions will be given.

3. DEFINITIONS

Let x_i = the absolute measurement or true value of the characteristic considered, where i represents the ith item in the sample

e_{ij} = the error in measurement of the ith item by the jth instrument, I_j

The observed values are then $x_i + e_{ij}$ and the mean measurement,

$$\bar{x} + \bar{e}_j = \frac{1}{n} \sum_{i=1}^{n} (x_i + e_{ij})$$

Taking

$$s_{uv} \quad \text{or} \quad s_{u,v} = \frac{1}{n-1} \sum_{i=1}^{n} (u_i - \bar{u})(v_i - \bar{v}),$$

it follows that

$$s_u^2 = s_{uu} = \frac{1}{n-1} \sum_{i=1}^{n} (u_i - \bar{u})^2.$$

Define also the following expected values:

$$E(x_i) = \mu, \qquad E(e_{ij}) = \epsilon_j$$
$$E(x_i - \mu)^2 = \sigma_x^2, \qquad E(e_{ij} - \epsilon_j)^2 = \sigma_{e_j}^2.$$

Hence,

$$E(s_{uu}) = \sigma_u^2.$$

4. ASSUMPTIONS

We will assume that x_i and e_{ij} are both normally distributed (although this is not necessary in so far as estimating variance only is concerned) and that the absolute values of the characteristic measured and the errors of measurement are statistically independent, i.e.

$$E(x_i^p \cdot e_{ij}^q) = E(x_i^p) \cdot E(e_{ij}^q).$$

In addition, it will be assumed that the errors of measurement of the different instruments are also statistically independent of each other, i.e.

$$E(e_{ij}^p \cdot e_{ik}^q) = E(e_{ij}^p) \cdot E(e_{ik}^q) \qquad j \neq k.$$

5. THE SEPARATION OF s_x^2 AND $s_{e_j}^2$ WITH TWO INSTRUMENTS

If two instruments I_1 and I_2 are used to make measurements on the same n items, the observed values can be represented symbolically as follows:

Measurements by I_1	Measurements by I_2
$x_1 + e_{11}$	$x_1 + e_{12}$
$x_2 + e_{21}$	$x_2 + e_{22}$
⋮	⋮
$x_i + e_{i1}$	$x_i + e_{i2}$
⋮	⋮
$x_n + e_{n1}$	$x_n + e_{n2}$

For the case of two instruments, there are two methods or computational procedures for estimating precision of measurement.

Method A_2:

As an estimate of the variance in errors of measurement of instrument I_1, we take[2]

$$\text{(1)} \qquad \text{est. } (\sigma_{e_1}^2) = s^2_{x+e_1} - s_{x+e_1,x+e_2}$$

where

$$s_{x+e_1,x+e_2} = \frac{1}{n-1}\sum_{i=1}^{n}\{(x_i + e_{i1}) - (\bar{x} + \bar{e}_1)\}\{(x_i + e_{i2}) - (\bar{x} + \bar{e}_2)\}$$

$$= \frac{1}{n(n-1)}\left\{n\sum_{i=1}^{n}(x_1 + e_{i1})(x_{i1} + e_{i2}) - \left[\sum_{i=1}^{n}(x_i + e_{i1})\right]\left[\sum_{i=1}^{n}(x_i + e_{i2})\right]\right\}$$

and as an estimate of the variance in errors of measurement of instrument I_2, we compute

$$\text{(2)} \qquad \text{est. } (\sigma_{e_2}^2) = s^2_{x+e_2} - s_{x+e_1,x+e_2}.$$

The estimates (1) and (2) are unbiased, since, for example,

$$\text{est. } (\sigma_{e_1}^2) = s_{e_1}^2 + s_{xe_1} - s_{xe_2} - s_{e_1e_2} \text{ symbolically}$$

and

$$E(s_{e_1}^2) = \sigma_{e_1}^2,$$

whereas

$$E(s_{xe_1}) = E(s_{xe_2}) = E(s_{e_1e_2}) = 0 \text{ under the assumptions.}$$

Method B_2:

It is remarked in passing that a technique of working with differences in corresponding measurements of the two instruments is of some interest. That is, if the measurements of I_2 are substracted from corresponding measurements of I_1, the result gives

$$\begin{matrix} e_{11} - e_{12} \\ e_{21} - e_{22} \\ \vdots \\ e_{i1} - e_{i2} \\ \vdots \\ e_{n1} - e_{n2}, \end{matrix}$$

[2] Estimates (1) and (2) are maximum likelihood estimates.

thus eliminating the true value of x_i, the characteristic measured. Using the above differences, an estimate of $\sigma_{e_1}^2$ is given by

$$\text{(3)} \qquad \text{est. } (\sigma_{e_1}^2) = \tfrac{1}{2}\left\{s^2_{x+e_1} - s^2_{x+e_2} + s^2_{e_1-e_2}\right\}.$$

It is easy to see that estimate (3) is symbolically

$$s_{e_1}^2 + s_{xe_1} - s_{xe_2} - s_{e_1e_2}$$

and hence precisely that given by (1).

In order to estimate the amount of variability in the product being measured, we may first add corresponding measurements of the two instruments, obtaining symbolically

$$\begin{array}{c} x_1 + e_{11} + x_1 + e_{12} \\ \vdots \\ x_i + e_{i1} + x_i + e_{i2} \\ \vdots \\ x_n + e_{n1} + x_n + e_{n2}. \end{array}$$

The variance of the above sums of readings is symbolically

$$4s_x^2 + s_{e_1}^2 + s_{e_2}^2 + 4s_{xe_1} + 4s_{xe_2} + 2s_{e_1e_2}$$

so that an estimate of the variability in the product, σ_x^2, is given by

$$\text{(4)} \qquad \text{est. } (\sigma_x^2) = \tfrac{1}{4}\left\{s^2_{x+e_1+x+e_2} - s^2_{e_1-e_2}\right\}.$$

Actually, however, the estimate of σ_x^2 is given simply by the covariance of the readings of I_1 and I_2:

$$\text{(4a)} \qquad \text{est. } (\sigma_x^2) = s_{x+e_1,x+e_2} \text{ (4 and 4a are identical).}$$

Estimate (4) or (4a) is obviously unbiased and can be shown to be the maximum likelihood estimate.

In order to determine the reliability of our estimates of precision in measurement, we examine the variances of the estimates. Now the population variance of (1) is given by

$$E\left\{\text{est. } (\sigma_{e_1}^2) - \sigma_{e_1}^2\right\}^2 = E\left\{s_{e_1}^2 + s_{xe_1} - s_{xe_2} - s_{e_1e_2}\right\}^2 - \sigma_{e_1}^4.$$

It is well-known[3] that we may write

$$E(s_{e_1}^4) - \sigma_{e_1}^4 = \frac{1}{n(n-1)}\left\{(n-1)\mu_{4:e_1} + (3-n)\sigma_{e_1}^4\right\}$$
$$= \frac{2}{n-1}\sigma_{e_1}^4$$

for a normal parent where $\mu_{4:e_1}$ is the fourth central moment of e_{i1}. Also

$$E(s_{xe_1}^2) = \frac{1}{n-1}\sigma_x^2\sigma_{e_1}^2,$$

$$E(s_{xe_2}^2) = \frac{1}{n-1}\sigma_x^2\sigma_{e_2}^2,$$

$$E(s_{e_1e_2}^2) = \frac{1}{n-1}\sigma_{e_1}^2\sigma_{e_2}^2$$

so that

$$E\left\{\text{est. } (\sigma_{e_1}^2) - \sigma_{e_1}^2\right\}^2$$
$$= \frac{2}{n-1}\sigma_{e_1}^4 + \frac{1}{n-1}(\sigma_x^2\sigma_{e_1}^2 + \sigma_x^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_2}^2). \tag{5}$$

Likewise, the population variance of the estimate of $\sigma_{e_2}^2$ is given by

$$E\left\{\text{est. } (\sigma_{e_2}^2) - \sigma_{e_2}^2\right\}^2$$
$$= \frac{2}{n-1}\sigma_{e_2}^4 + \frac{1}{n-1}(\sigma_x^2\sigma_{e_1}^2 + \sigma_x^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_2}^2) \tag{6}$$

and the population variance of the estimate of product variability is given by

$$E\left\{\text{est. } (\sigma_x^2) - \sigma_x^2\right\}^2$$
$$= \frac{2}{n-1}\sigma_x^2 + \frac{1}{n-1}\left\{\sigma_x^2\sigma_{e_1}^2 + \sigma_x^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_2}^2\right\}. \tag{7}$$

It is to be noted that the variance of the estimate, est. $(\sigma_{e_1}^2)$ or est. $(\sigma_{e_2}^2)$ depends on (*a*) σ_x^2, the variance in the characteristic measured,

[3] See, for example, R. A. Fisher, "Moments and Product Moments of Sampling Distributions." *Proceedings of the London Mathematical Society*, Vol. 30, 1929, pp 215–218.

(*b*) $\sigma_{e_1}^2$, the variance of the errors of measurement of instrument I_1, (*c*) $\sigma_{e_2}^2$, the variance of the errors of measurement of instrument I_2 and (*d*) n, the number of observations or the sample size. It is seen, therefore, that in order to obtain a precise estimate of $\sigma_{e_1}^2$ when using only two instruments, the variation in the characteristic measured, i.e. σ_x^2, should be held to a reasonable minimum or the sample size, n, should be sufficiently large.

If the variation in the characteristic measured is zero (or if we measure the same item over and over again), i.e. if $\sigma_x^2=0$, then one could compute

$$\frac{1}{n-1}\sum_{i=1}^{n}(e_{i1}-\bar{e}_1)^2$$

directly, and this would give an estimate of $\sigma_{e_1}^2$ with variance equal to

$$\frac{2}{n-1}\sigma_{e_1}^4.$$

Apparently, in employing two instruments, there are only two computational procedures of interest for separating the variability in the product from the variance in the errors of measurement and both methods give the same estimate. In using either method, however, it is possible to estimate $\sigma_{e_1}^2$, $\sigma_{e_2}^2$ and σ_x^2 and thus determine from the relative order of magnitude of these quantities whether the instruments are sufficiently precise to use in taking the required measurements.

It is pointed out that the average observed value of I_1 is $\bar{x}+\bar{e}_1$ and the average reading of I_2 is $\bar{x}+\bar{e}_2$, so that upon subtracting, $\bar{e}_1-\bar{e}_2$ is obtained, the average difference in the errors of measurement of I_1 and I_2. Of course, $\bar{e}_1$ and $\bar{e}_2$ are inseparable here also and estimates of either depend upon calibration of I_1, I_2 or both against a standard.

6. THE SEPARATION OF s_x^2 AND $s_{e_j}^2$ WITH THREE INSTRUMENTS[4]

If three instruments I_1, I_2 and I_3 are used to make measurements on the same n items, the observed values can be represented as:

[4] The questions of optimum estimates and efficiency of methods suggested below for three or more instruments are not answered in this paper. It is believed, however, that the suggested techniques show promise of providing good, workable estimates. It is hoped that a more complete investigation of this problem will be undertaken in the near future and that others will interest themselves in this rather important study.

Measurements by I_1	Measurements by I_2	Measurements by I_3
$x_1 + e_{11}$	$x_1 + e_{12}$	$x_1 + e_{13}$
$x_2 + e_{21}$	$x_2 + e_{22}$	$x_2 + e_{23}$
$\vdots$	$\vdots$	$\vdots$
$x_n + e_{n1}$	$x_n + e_{n2}$	$x_n + e_{n3}$

For the case of three instruments two techniques will be considered for estimating $\sigma_{e_1}^2$, $\sigma_{e_2}^2$, and $\sigma_{e_3}^2$, the variances in the errors of measurement of instruments I_1, I_2, and I_3.

Method A_3:

Two computational procedures giving rise to the same estimate will be given here.

First, an estimate of $\sigma_{e_1}^2$ is given by

$$\text{(8)} \qquad \text{est. } (\sigma_{e_1}^2) = s^2_{x+e_1} - s_{x+e_1,x+e_2} - s_{x+e_1,x+e_3} + s_{x+e_2,x+e_3}.$$

Also, in some particular applications it may be enlightening to eliminate the value of the characteristic measured by working with differences. That is, for each item measured, form the three columns of differences in observed values of the instruments taken two at a time. Thus,

Differences in Readings, $I_1 - I_2$	Differences in Readings, $I_1 - I_3$	Differences in Readings, $I_2 - I_3$
$e_{11} + e_{12}$	$e_{11} - e_{13}$	$e_{12} - e_{13}$
$e_{21} - e_{22}$	$e_{21} - e_{23}$	$e_{22} - e_{23}$
$\vdots$	$\vdots$	$\vdots$
$e_{n1} - e_{n2}$	$e_{n1} - e_{n3}$	$e_{n2} - e_{n3}$

The variances of these three columns will be symbolically

$$s^2_{e_1-e_2} = s_{e_1}^2 - 2s_{e_1e_2} + s_{e_2}^2, \qquad s^2_{e_1-e_3} = s_{e_1}^2 - 2s_{e_1e_3} + s_{e_3}^2,$$
$$s^2_{e_2-e_3} = s_{e_2}^2 - 2s_{e_2e_3} + s_{e_3}^2.$$

Hence, as an estimate of $\sigma_{e_1}^2$, take

$$\text{(9)} \qquad \text{est. } (\sigma_{e_1}^2) = \tfrac{1}{2}\left\{s^2_{e_1-e_2} + s^2_{e_1-e_3} - s^2_{e_2-e_3}\right\}.$$

Estimates (8) and (9) are identical, both being equal to

$$s_{e_1}^2 - s_{e_1e_2} - s_{e_1e_3} + s_{e_2e_3} \text{ symbolically.}$$

Estimates (8) and (9) are unbiased and also free of the characteristic measured.

As an estimate of $\sigma_{e_2}^2$, form

$$\text{(10)} \qquad \text{est. } (\sigma_{e_2}^2) = s^2_{x+e_2} - s_{x+e_1,x+e_2} + s_{x+e_1,x+e_3} - s_{x+e_2,x+e_3}$$

or carry out a computation similar to (9).

As an estimate of $\sigma_{e_3}^2$, form

$$\text{(11)} \qquad \text{est. } (\sigma_{e_3}^2) = s^2_{x+e_3} + s_{x+e_1,x+e_2} - s_{x+e_1,x+e_3} - s_{x+e_2,x+e_3}$$

or carry out a computation similar to (9).

The variance of estimate (8) or (9) is given by

$$\begin{aligned} E\{\text{est. } (\sigma_{e_1}^2) - \sigma_{e_1}^2\}^2 &= E\{\text{est. } (\sigma_{e_1}^2)\}^2 - \sigma_{e_1}^4 \\ &= E\{s_{e_1}^2 - s_{e_1e_2} - s_{e_1e_3} + s_{e_2e_3}\}^2 - \sigma_e^4. \end{aligned}$$

In an analogous manner to that for determining the variance of an estimate in the case of two instruments, it is easily seen that for three instruments

$$\text{(12)} \qquad \begin{aligned} &E\{\text{est. } (\sigma_{e_1}^2) - \sigma_e^2\}^2 \\ &\quad = \frac{2}{n-1}\sigma_{e_1}^4 + \frac{1}{n-1}(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2). \end{aligned}$$

Likewise, the variance of the estimate (10) is

$$\text{(13)} \qquad \frac{2}{n-1}\sigma_{e_2}^4 + \frac{1}{n-1}(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2)$$

and the variance of (11) is given by

$$\text{(14)} \qquad \frac{2}{n-1}\sigma_{e_3}^4 + \frac{1}{n-1}(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2).$$

Thus, the variances of the estimates of variation in errors of measurement obtained above for three instruments are entirely free of σ_x^2, the variability in the characteristic measured. Apparently, at least three instruments are needed to eliminate the characteristic x.

Estimate (8) using three instruments will be more precise than estimate (1) employing two instruments provided

$$\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2 < \sigma_x^2\sigma_{e_1}^2 + \sigma_x^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_2}^2$$

i.e. if

$$\sigma_{e_3}^2 < \sigma_x^2.$$

Since in general the variance in errors of measurement should be smaller than the variance in absolute values of the characteristic measured, it would appear that there is some advantage in the use of the additional measuring instrument, I_3 for the purpose of estimating $\sigma_{e_1}^2$, $\sigma_{e_2}^2$, or $\sigma_{e_3}^2$.

Method B_3:[5]

This technique involves the variance of the readings of I_1 and the average of the three co-variances which can be formed from the three sets of observed values for instruments I_1, I_2, and I_3.

Now the co-variance of the I_1 and I_2 readings is

$$s_{x+e_1,x+e_2} = s_x^2 + s_{xe_1} + s_{xe_2} + s_{e_1e_2} \text{ symbolically.}$$

The co-variance of the I_2 and I_3 readings is symbolically

$$s_x^2 + s_{xe_2} + s_{xe_3} + s_{e_2e_3}$$

and that of the I_1 and I_3 readings is

$$s_x^2 + s_{xe_1} + s_{xe_3} + s_{e_1e_3}.$$

The average of these three co-variances is symbolically

$$s_x^2 + \tfrac{2}{3}s_{xe_1} + \tfrac{2}{3}s_{xe_2} + \tfrac{2}{3}s_{xe_3} + \tfrac{1}{3}s_{e_1e_2} + \tfrac{1}{3}s_{e_2e_3} + \tfrac{1}{3}s_{e_1e_3}.$$

As previously stated, the variance of the observations with instrument I_1 is symbolically

$$s^2{}_{x+e_1} = s_x^2 + 2s_{xe_1} + s_{e_1}^2.$$

The latter variance minus the average co-variance gives as an estimate of $\sigma_{e_1}^2$

$$\begin{aligned} \text{est. } (\sigma_{e_1}^2) &= s^2{}_{x+e_1} - \tfrac{1}{3}\left\{s_{x+e_1,x+e_2} + s_{x+e_1,x+e_3} + s_{x+e_2,x+e_3}\right\} \\ &= s_{e_1}^2 + \tfrac{4}{3}s_{xe_1} - \tfrac{2}{3}s_{xe_2} - \tfrac{2}{3}s_{xe_3} - \tfrac{1}{3}s_{e_1e_2} - \tfrac{1}{3}s_{e_1e_3} \\ &\quad - \tfrac{1}{3}s_{e_2e_3} \text{ symbolically} \end{aligned} \tag{15}$$

and the variance of this estimate is, in a manner analogous to that previously indicated, given by

$$\frac{2}{n-1}\sigma_{e_1}^4 + \frac{1}{n-1}\left\{\tfrac{16}{9}\sigma_x^2\sigma_{e_1}^2 + \tfrac{4}{9}\sigma_x^2\sigma_{e_2}^2 + \tfrac{4}{9}\sigma_x^2\sigma_{e_3}^2 + \tfrac{1}{9}\sigma_{e_1}^2\sigma_{e_2}^2 + \tfrac{1}{9}\sigma_{e_1}^2\sigma_{e_3}^2 + \tfrac{1}{9}\sigma_{e_2}^2\sigma_{e_3}^2\right\}. \tag{16}$$

[5] Mr. H A. Noble of the Ballistic Research Laboratory, using correlation coefficients, proposed a technique for estimating $\sigma_{e_1}^2$ precisely equivalent to Method B_3 and also suggested the problem of comparing the variance of Method B_3 with that of Method A_3 when three instruments are employed.

Recalling that Method A_3 for the case of three instruments gave an estimate whose variance is given by

$$\frac{2}{n-1}\sigma_{e_1}^4 + \frac{1}{n-1}(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2),$$

it may be seen that Method B_3 will not give an estimate of $\sigma_{e_1}^2$ as precise as that of Method A_3 unless

$$\sigma_x^2 < \frac{2(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2)}{4\sigma_{e_1}^4 + \sigma_{e_2}^2 + \sigma_{e_3}^2}.$$

Should it be assumed that $\sigma_{e_1}^2 \leqq \sigma_{e_2}^2 \leqq \sigma_{e_3}^2$, then replacing $\sigma_{e_1}^2$ and $\sigma_{e_2}^2$ by $\sigma_{e_3}^2$ in the numerator and replacing $\sigma_{e_2}^2$ and $\sigma_{e_3}^2$ by $\sigma_{e_1}^2$ in the denominator above, one arrives at the liberal inequality for σ_x^2:

$$\sigma_x^2 < \frac{\sigma_{e_3}^4}{\sigma_{e_1}^2}.$$

But even relaxing the inequality thus and under the assumption, $\sigma_{e_1}^2 \leqq \sigma_{e_3}^2$, then the estimate of Method B_3 therefore will still not be as precise as that of Method A_3 unless

$$\sigma_x^2 < \sigma_{e_3}^2.$$

Generally speaking, however, it should be desirable for the precision of measurement or variance in errors of measurement (such as $\sigma_{e_1}^2$, $\sigma_{e_2}^2$, or $\sigma_{e_3}^2$) to be considerably smaller than the variance in the characteristic measured, σ_x^2. The appropriate relation between σ_x^2 and $\sigma_{e_j}^2$ depends upon the purpose for which the measurements are taken and the cost of insuring that $\sigma_{e_j}^2$ is sufficiently small relative to σ_x^2.

To obtain an estimate of product variability when three instruments are used to measure the characteristic x, the average covariance of the readings of I_1, I_2 and I_3 is computed. That is

$$\begin{aligned}(17)\qquad \text{est. } (\sigma_x^2) &= \tfrac{1}{3}\left\{s_{x+e_1,x+e_2} + s_{x+e_1,x+e_3} + s_{x+e_2,x+e_3}\right\} \\ &= s_x^2 + \tfrac{2}{3}s_{xe_1} + \tfrac{2}{3}s_{xe_2} + \tfrac{2}{3}s_{xe_3} + \tfrac{1}{3}s_{e_1e_2} + \tfrac{1}{3}s_{e_1e_3} \\ &\quad + \tfrac{1}{3}s_{e_2e_3} \text{ symbolically}\end{aligned}$$

with variance therefore equal to

$$\begin{aligned}(18)\qquad &\frac{2}{n-1}\sigma_x^4 + \frac{1}{n-1}\left\{\tfrac{4}{9}(\sigma_x^2\sigma_{e_1}^2 + \sigma_x^2\sigma_{e_2}^2 + \sigma_x^2\sigma_{e_3}^2)\right. \\ &\qquad\qquad \left. + \tfrac{1}{9}(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2)\right\}.\end{aligned}$$

In case it may be desirable to list the average observed values of the characteristic measured, as in acceptance tests of products (see example at end of paper), then σ_x^2 may be estimated by the following method which involves variances of differences in readings of the three instruments. The mean reading of the three instruments on the ith item is symbolically

$$x_i + \tfrac{1}{3}e_{i1} + \tfrac{1}{3}e_{i2} + \tfrac{1}{3}e_{i3}$$

and the variance of such means is symbolically

$$s_x^2 + \tfrac{1}{9}(s_{e_1}^2 + s_{e_2}^2 + s_{e_3}^2) + \tfrac{2}{3}(s_{xe_1} + s_{xe_2} + s_{xe_3}) + \tfrac{2}{9}(s_{e_1e_2} + s_{e_1e_3} + s_{e_2e_3}).$$

Subtracting 1/18th the sum of variances of differences in readings of the instruments taken two at a time, one obtains the unbiased estimate

$$\text{(19)} \quad \text{est. } (\sigma_x^2) = s^2_{x+\frac{1}{3}e_1+\frac{1}{3}e_2+\frac{1}{3}e_3} - \tfrac{1}{18}\left\{s^2_{e_1-e_2} + s^2_{e_1-e_3} + s^2_{e_2-e_3}\right\}.$$

If it were desirable to work with sums of the readings of I_1, I_2 and I_3 of each item, (19) may, of course, be rewritten as

$$\text{(20)} \quad \text{est. } (\sigma_x^2) = \tfrac{1}{9}\left\{s^2_{3x+e_1+e_2+e_3} - \tfrac{1}{2}\left[s^2_{e_1-e_2} + s^2_{e_1-e_3} + s^2_{e_2-e_3}\right]\right\}.$$

It is remarked that (17), (19) and (20) are identical estimates of product variability.

It may be noted that the average measurements of the individual instruments I_1, I_2 and I_3 are

$$\bar{x} + \bar{e}_1, \qquad \bar{x} + \bar{e}_2, \qquad \bar{x} + \bar{e}_3$$

respectively and the differences in average errors of measurements of the instruments in pairs are

$$\bar{e}_1 - \bar{e}_2, \qquad \bar{e}_1 - \bar{e}_3, \qquad \bar{e}_2 - \bar{e}_3$$

which, of course, can be computed from the data giving rise to three equations and three unknowns

$$\bar{e}_1 - \bar{e}_2 = c_1$$
$$\bar{e}_1 - \bar{e}_3 = c_2$$
$$\bar{e}_2 - \bar{e}_3 = c_3.$$

However, the point of interest here is that although c_1, c_2 and c_3 are known from the data, the above simultaneous equations cannot be solved for the average errors of measurement $\bar{e}_1$, $\bar{e}_2$ and $\bar{e}_3$ since the equations are obviously linearly dependent. Of course, the size of $\bar{e}_1$, $\bar{e}_2$ and $\bar{e}_3$ may be determined relative to a standard.

7. THE SEPARATION OF s_x^2 AND $s_{e_j}^2$ WITH FOUR INSTRUMENTS

If four instruments I_1, I_2, I_3 and I_4 are used to make measurements on the same n items, the observed values or readings of the instruments can be represented as follows:

Measurements by I_1	Measurements by I_2	Measurements by I_3	Measurements by I_4
$x_1 + e_{11}$	$x_1 + e_{12}$	$x_1 + e_{13}$	$x_1 + e_{14}$
$x_2 + e_{21}$	$x_2 + e_{22}$	$x_2 + e_{23}$	$x_2 + e_{24}$
$\vdots$	$\vdots$	$\vdots$	$\vdots$
$x_n + e_{n1}$	$x_n + e_{n2}$	$x_n + e_{n3}$	$x_n + e_{n4}$

From the discussion so far, it is apparent that $\sigma_{e_1}^2$ and σ_x^2 may be estimated by considering the readings of I_1 with any one of the instruments I_2, I_3 and I_4—similarly for estimating $\sigma_{e_2}^2$, $\sigma_{e_3}^2$ and $\sigma_{e_4}^2$. Also, the variances in errors of measurement and variance in the characteristic measured may be estimated by considering the readings of any three of the four instruments at a time. These two problems have already been treated above. It remains, therefore, to consider simultaneously the results of measurement of all four instruments and to compare the precision of estimates thus obtained with that for the case involving three instruments.

Method A_4:

Using the variance of the readings of I_1 and the covariances of the readings of the four instruments taken two at a time, an estimate of $\sigma_{e_1}^2$ is given by

$$\text{(21)} \quad \begin{aligned} \text{est. } (\sigma_{e_1}^2) = {} & s^2_{x+e_1} - \tfrac{2}{3}(s_{x+e_1,x+e_2} + s_{x+e_1,x+e_3} + s_{x+e_1,x+e_4}) \\ & + \tfrac{1}{3}(s_{x+e_2,x+e_3} + s_{x+e_2,x+e_4} + s_{x+e_3,x+e_4}). \end{aligned}$$

On the other hand, by forming the six columns of differences in readings of the four instruments taken two at a time, an estimate of $\sigma_{e_1}^2$ is given by

$$\text{(22)} \quad \text{est. } (\sigma_{e_1}^2) = \tfrac{1}{3}\left\{s^2_{e_1-e_2} + s^2_{e_1-e_3} + s^2_{e_1-e_4} - \tfrac{1}{2}\left[s^2_{e_2-e_3} + s^2_{e_2-e_4} + s^2_{e_3-e_4}\right]\right\}.$$

Estimates (21) and (22) are equivalent and symbolically equal to

$$s_{e_1}^2 - \tfrac{2}{3}(s_{e_1e_2} + s_{e_1e_3} + s_{e_1e_4}) + \tfrac{1}{3}(s_{e_2e_3} + s_{e_2e_4} + s_{e_3e_4}).$$

Consequently, the variance of estimate (21) or (22) is

$$(23)\quad \frac{2}{n-1}\sigma_{e_1}^4 + \frac{1}{n-1}\left\{\tfrac{4}{9}(\sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_1}^2\sigma_{e_4}^2) + \tfrac{1}{9}(\sigma_{e_2}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_4}^2 + \sigma_{e_3}^2\sigma_{e_4}^2)\right\}.$$

Estimates of $\sigma_{e_2}^2$, $\sigma_{e_3}^2$ and $\sigma_{e_4}^2$ by the above procedures and the variances of such estimates can be formed simply by rotating the subscripts e_1, e_2, e_3 and e_4 in (21) or (22) and (23) appropriately.

The estimate (21) or (22) of $\sigma_{e_1}^2$ and its variance (23) are entirely free of x, the characteristic measured, as is the estimate (8) or (9) employing three instruments. Estimate (21) or (22) using four instruments will be more precise than estimate (8) or (9) using three instruments provided

$$\tfrac{4}{9}\sigma_{e_1}^2\sigma_{e_2}^2 + \tfrac{4}{9}\sigma_{e_1}^2\sigma_{e_3}^2 + \tfrac{4}{9}\sigma_{e_1}^2\sigma_{e_4}^2 + \tfrac{1}{9}\sigma_{e_2}^2\sigma_{e_3}^2 + \tfrac{1}{9}\sigma_{e_2}^2\sigma_{e_4}^2 + \tfrac{1}{9}\sigma_{e_3}^2\sigma_{e_4}^2 < \sigma_{e_1}^2\sigma_{e_2}^2 + \sigma_{e_1}^2\sigma_{e_3}^2 + \sigma_{e_2}^2\sigma_{e_3}^2$$

i.e. provided

$$\sigma_{e_4}^2 < \frac{5\sigma_{e_1}^2\sigma_{e_2}^2 + 5\sigma_{e_1}^2\sigma_{e_3}^2 + 8\sigma_{e_2}^2\sigma_{e_3}^2}{4\sigma_{e_1}^2 + \sigma_{e_2}^2 + \sigma_{e_3}^2}.$$

This condition appears easy to meet, since, for example, if $\sigma_{e_1}^2=\sigma_{e_2}^2=\sigma_{e_3}^2=\sigma^2$, then one has only to be sure that

$$\sigma_{e_4}^2 < 3\sigma^2.$$

Moreover, if it is assumed that $\sigma_{e_1}^2 \leqq \sigma_{e_2}^2 \leqq \sigma_{e_3}^2$ then estimate (21) will surely be more precise than estimate (8) or (9) so long as

$$\sigma_{e_4}^2 < \frac{5\sigma_{e_1}^2\sigma_{e_1}^2 + 8\sigma_{e_1}^2\sigma_{e_1}^2 + 5\sigma_{e_1}^2\sigma_{e_1}^2}{4\sigma_{e_3}^2 + \sigma_{e_3}^2 + \sigma_{e_3}^2} \leqq \frac{3\sigma_{e_1}^4}{\sigma_{e_3}^2}$$

or if

$$\sigma_{e_4}^2 < 3\sigma_{e_1}^2.$$

It is believed that this condition may be met easily in practice. Consequently, the use of an additional measuring instrument will apparently decrease the variances of the estimates of precision of measurement.

8. THE SEPARATION OF s_x^2 AND s_j^2 WITH N INSTRUMENTS

Defining e_{ij} as before to be the error in measurement of the ith item ($i=1, 2, \cdots, n$) by the jth instrument, it is easy to extend the above

results to any number, N, of instruments which are employed to measure the items, x_i. An estimate of the variance in errors of measurement of instrument I_1 is for $N \geqq 3$ given by

$$\text{est. } (\sigma_{e_1}^2) = s^2_{x+e_1} - \frac{2}{N-1}\left\{\sum_{r=2}^{N} s_{x+e_1,x+e_r}\right\} + \frac{2}{(N-1)(N-2)}\left\{\sum_{2\leqq j<k}^{k=N} s_{x+e_j,x+e_k}\right\} \tag{24}$$

which is symbolically equal to

$$s_{e_1}^2 - \frac{2}{N-1}\sum_{r=2}^{N} s_{e_1e_r} + \frac{2}{(N-1)(N-2)}\sum_{2\leqq j<k}^{k=N} s_{e_je_k}$$

so that the variance of (24) is given by

$$\frac{2}{n-1}\sigma_{e_1}^4 + \frac{1}{n-1}\left\{\frac{4}{(N-1)^2}\sum_{r=2}^{N}\sigma_{e_1}^2\sigma_{e_r}^2 + \frac{4}{(N-1)^2(N-2)^2}\sum_{2\leqq j<k}^{k=N}\sigma_{e_j}^2\sigma_{e_k}^2\right\}. \tag{25}$$

Formulae for estimates of $\sigma_{e_2}^2$, $\sigma_{e_3}^2$, etc. and variances of such estimates may be found by obvious rotations of the subscripts.

The estimate of variability in the product is taken as the average of the covariances, i.e.

$$\text{est. } (\sigma_x^2) = \frac{2}{N(N-1)}\sum_{1\leqq r<s}^{s=N} s_{x+e_r,x+e_s} \qquad (N \geqq 2)$$
$$= s_x^2 + \frac{2}{N}\sum_{r=1}^{N} s_{xe_r} + \frac{2}{N(N-1)}\sum_{1\leqq r<s}^{s=N} s_{e_re_s} \text{ symbolically} \tag{26}$$

so that the variance of estimate (26) is given by

$$\frac{2}{n-1}\sigma_x^4 + \frac{1}{n-1}\left\{\frac{4}{N^2}\sigma_x^2\sum_{r=1}^{N}\sigma_{e_r}^2 + \frac{4}{N^2(N-1)^2}\sum_{1\leqq r<s}^{s=N}\sigma_{e_r}^2\sigma_{e_s}^2\right\}. \tag{27}$$

Should it be desirable to work with differences in errors of measurements of the instruments taken two at a time, an estimate of $\sigma_{e_1}^2$ is given by

$$(28)\quad \text{est. } (\sigma_{e_1}^2) = \frac{1}{N-1}\left\{\sum_{r=2}^{N} s^2_{e_1-e_r} - \frac{1}{N-2}\sum_{2\leq j<k}^{k=N} s^2_{e_j-e_k}\right\}. \quad (N \geq 3)$$

Estimates (24) and (28) are identical and hence have the same variance.

Using the average measurement of the N instruments on x_i and differences in errors of measurement, an estimate of product variability is

$$(29)\quad \text{est. } (\sigma_x^2) = s^2_{x+(1/N)(e_1+e_2+\cdots+e_N)} - \frac{1}{N^2(N-1)}\sum_{1\leq r<s}^{N} s^2_{e_r-e_s}.$$

On the other hand, using the sum of the measurements of the N instruments on x_i and columns of differences in errors of measurements, (29) may be rewritten as

$$(30)\quad \text{est. } (\sigma_x^2) = \frac{1}{N^2}\left\{s^2_{Nx+e_1+e_2+\cdots+e_n} - \frac{1}{N-1}\sum_{1\leq r<s}^{N} s^2_{e_r-e_s}\right\}.$$

It is easy to show that (29), (30) and (26) are identical estimates.

9. EXAMPLE ON ESTIMATING PRECISION OF MEASUREMENT AND PRODUCT VARIABILITY[6]

In Table I there are listed the individual burning times of powder train fuzes as measured by each of three observers on 30 rounds of ammunition which were fired from a gun. The fuzes were all set for a burning time of ten seconds.

The burning time of a fuze is defined as the interval of time which elapses from the instant the projectile leaves the gun muzzle until the fuze functions the projectile. The times given in Table I are measured by means of electric clocks. A switch on the gun muzzle starts three different electric clocks as the gun is fired and each observer stops his clock the instant he sees the flash or burst of an individual round. Each timer, of course, stops his clock independently of the other two timers. In Table I is given also the mean time of the three observers on individual rounds. The average time-to-burst of the 30 fuzes is taken as the average of the 30 mean times with the effect of reaction time (which is known accurately) subtracted therefrom. The problem here is to

[6] The data for this example are given also in Colonel L. E. Simon's paper, "On the Relation of Instrumentation to Quality Control," *Instruments*, vol. 19, November, 1946.

TABLE I

Fuze Burning Times (seconds)			Mean Times (seconds)
Observer A	Observer B	Observer C	
10.10	10.07	10.07	10.080
9.98	9.90	9.90	9.927
9.89	9.85	9.86	9.867
9.79	9.71	9.70	9.733
9.67	9.65	9.65	9.657
9.89	9.83	9.83	9.850
9.82	9.75	9.79	9.787
9.59	9.56	9.59	9.580
9.76	9.68	9.72	9.720
9.93	9.89	9.92	9.913
9.62	9.61	9.64	9.623
10.24	10.23	10.24	10.237
9.84	9.83	9.86	9.843
9.62	9.58	9.63	9.610
9.60	9.60	9.65	9.617
9.74	9.73	9.74	9.737
10.32	10.32	10.34	10.327
9.86	9.86	9.86	9.860
10.01	lost	10.03	10.020
9.65	9.64	9.65	9.647
9.50	9.49	9.50	9.497
9.56	9.56	9.55	9.557
9.54	9.53	9.54	9.537
9.89	9.89	9.88	9.887
9.53	9.52	9.51	9.520
9.52	9.52	9.53	9.523
9.44	9.43	9.45	9.440
9.67	9.67	9.67	9.670
9.77	9.76	9.78	9.770
9.86	9.84	9.86	9.853

determine whether the electric clock is a satisfactory instrument for measuring burning times, provided the electric clocks are properly calibrated with regard to average time so that the systematic error of measurement may be reconciled. This question is answered by comparing the variance in errors of measurement with the variance in burning times of the fuzes.

In Table II there are given the algebraic differences between times of the three observers A, B, and C on each round.

It is to be noted that any one of the differences listed is not influenced by the level of burning time of an individual fuze and represents the actual difference in errors of measurement of the two observers involved. Thus, the variance of each of the columns headed A–B, B–C, and A–C, gives the variability of the difference in errors of measurement of the two observers compared. Therefore, taking the variances

TABLE II
DIFFERENCE BETWEEN OBSERVERS' TIMES
(SECONDS)

A–B	*B–C*	*A–C*
+.03	0	+.03
+.08	0	+.08
+.04	−.01	+.03
+.08	+.01	+.09
+.02	0	+.02
+.06	0	+.06
+.07	−.04	+.03
+.03	−.03	0
+.08	−.04	+.04
+.04	−.03	+.01
+.01	−.03	−.02
+.01	−.01	0
+.01	−.03	−.02
+.04	−.05	−.01
0	−.05	−.05
+.01	−.01	0
0	−.02	−.02
0	0	0
—	—	−.02
+.01	−.01	0
+.01	−.01	0
0	+.01	+.01
+.01	−.01	0
0	+.01	+.01
+.01	+.01	+.02
0	−.01	−.01
+.01	−.02	−.01
0	0	0
+.01	−.02	−.01
+.02	−.02	0

of the three columns in Table II, the following three equations are arrived at:

$$s^2_{e_1-e_2} = .0007030 \text{ sec.}^2$$

(a)
$$s^2_{e_1-e_3} = .0008878 \text{ sec.}^2$$

$$s^2_{e_2-e_3} = .0003108 \text{ sec.}^2$$

where e_1, e_2 and e_3 represent the errors of measurement of the observers A, B, and C respectively.

Using formulae similar to (9) of the paper for estimates of $\sigma_{e_1}^2$, $\sigma_{e_2}^2$, and $\sigma_{e_3}^2$ it is found that

$$\sigma_{e_1} = .0253 \text{ sec.}$$

(b)
$$\sigma_{e_2} = .0079 \text{ sec.}$$

$$\sigma_{e_3} = .0157 \text{ sec.}$$

The above figures give a direct comparison of the ability or precision of each of the observers. It is seen that observer B is perhaps the best of the three since his precision of measurement is given by a standard deviation of only .0079 second, whereas observer A is the poorest timer of the three. As a matter of fact, one can look at the columns in Table II headed A–B and A–C and note that for the first 10 rounds or so observer A had a definite lag in stopping his clock as compared to the other two observers.

An estimate of σ_x^2 is obtained by using formula (19), i.e. by subtracting 1/18th of the sum of the variances of differences (a) from the variance of mean times in column four of Table I. Thus,

$$\text{est. } (\sigma_x^2) = .046098 - \tfrac{1}{18}\{.0007030 + .0008878 + .0003108\}$$
$$= .04599$$

or $$\sigma_x = .2145.$$

Thus, it is seen that the variation in burning times, σ_x, is about 8.5 times the variance in errors, σ_{e_1}, for the poorest operator and about 12 times the average variability in errors of measurement of the three operators combined. Consequently, it is found that the combination of electric clocks and operators provides an adequate measuring instrument.

10. SIGNIFICANCE TESTS*

The problems of significance tests for comparing σ_x^2, $\sigma_{e_1}^2$, $\sigma_{e_2}^2$, etc. and judging whether $\epsilon_1 - \epsilon_2$, $\epsilon_1 - \epsilon_3$, $\cdots$, $\epsilon_2 - \epsilon_3$, $\cdots$, $\epsilon_{N-1} - \epsilon_N$ differ significantly from zero need to be investigated, although certain rough or approximate tests may be evident to the reader. In this connection, it is remarked, for example, that for the case of three instruments if $\sigma_{e_1}^2$, $\sigma_{e_2}^2$ and $\sigma_{e_3}^2$ are of the same order of magnitude, then the approximate number of degrees of freedom for the est. $(\sigma_{e_1}^2)$, est. $(\sigma_{e_2}^2)$ or est. $(\sigma_{e_3}^2)$ is $\frac{2}{3}(n-1)$. However, the reader is warned that questions of dependence among the estimates need to be investigated since they may have an important bearing on significance tests. It is hoped that an investigation of these problems can be undertaken in the near future.**

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* While this paper was being processed for publication, the author developed sequential probability ratio tests for the use involving two instruments.

** The author is indebted to Professor W. G. Cochran and referees of this paper for valuable suggestions in preparation of the manuscript.

LIFE COMPANY INVESTMENTS AND THE CAPITAL MARKETS*

FERGUS J. MCDIARMID

The Lincoln National Life Insurance Company

1. Savings made through life insurance are at this time the most common by far of all types of individual savings. Life insurance accumulations at the present time account for something around one-fourth of the total savings of individuals.

2. Savings made through life insurance are a very stable form of savings, varying less with business conditions than other forms of monetary savings.

3. At the present time, life insurance savings form by far the most important source of capital which seeks mainly a secure rate of return and safety of principal, rather than tax exemption or chance for capital appreciation. Traditionally they have been invested very largely in evidences of long term debt.

4. In the decade prior to the war, life company funds seemed to be outgrowing traditional investment outlets.

5. However, at the present time this trend seems to be in reverse, with investment outlets, even in traditional form, becoming rather plentiful. With declining savings, higher prices, and a huge real capital demand both here and abroad, we seem to have entered a period of real capital scarcity.

6. The main new outlet developed in recent years for life insurance funds has been direct investment in real estate, both housing and commercial.

7. The tendency for life insurance companies to invest largely in debt media may create a problem from the point of view of some industries which should be financed largely through equities.

GROWTH OF LIFE INSURANCE ASSETS

THE ASSETS of life insurance companies domiciled in the United States passed the $50 billion mark late in 1947, and they are increasing at a net rate in excess of $3 billion a year. This represents a doubling of the prewar rate of asset increase, a rate which still lags somewhat behind the increase in the national income over that period. Not only does this building up of life company assets create an investment problem of great magnitude, but it has an important influence on the capital markets in determining the types of securities through which capital can most readily be raised. Also the acceptability of an industry

* A paper presented at the 107th Annual Meeting of the American Statistical Association, New York City, December 30, 1947.

as a medium for the investment of life insurance funds may determine the relative ease with which it can raise capital in any form.

LIFE INSURANCE FUNDS IN RELATION TO TOTAL SAVINGS

Just how important is the flow of monetary saving through the life insurance companies, in relation to total saving by our people? The sum total of saving by individuals has been estimated by the Department of Commerce and the Securities and Exchange Commission. Each year's saving represents the sum of a number of items, the main ones being the increase in holdings of currency and bank deposits, the increase in insurance and pension reserves, the increase in security holdings, including U. S. Savings Bonds, the increase in the net equity in dwellings, and the decrease in consumer debt.

In the boom year 1929, the increase in life insurance reserves accounted for over 40 per cent of the total estimated net savings of individuals. During the decade of the thirties, the life insurance reserve increase slowed down some; but it constituted a rising proportion of total savings of individuals, averaging about three-fourths of the total estimate of these during that period. During the war years, the total of individual saving was greatly swollen; and life insurance accumulations, although rising, did decline as a proportion of total savings to less than 10 per cent of the total estimate in 1944. Since they reached their wartime peak in that year, net savings of individuals have declined very sharply, while life insurance assets have continued to grow. As a result, life insurance accounted for 22 per cent of the total saving flow in 1946. Because of the continued decline in the total of new saving by individuals, probably because of high living costs and the greater availability of durable consumer goods, life insurance funds probably will increase as a proportion of the total in 1947 and may very well account for at least one-fourth of total individual saving.

Certain features of the American economy tend to increase the relative importance of savings made through life insurance. It has become an increasingly urbanized economy and one of continuously greater specialization. To an increasing extent, the tools of production are financed and owned on a collective basis rather than through direct ownership by individuals. An expanding proportion of savings takes place through monetary channels rather than through the direct ownership of property.

Also, because of ascending income tax schedules on high incomes and the enlargement of the middle class group from below by higher real

wage levels, a large and increasing proportion of total savings is now derived from people in the middle income brackets. This is the group which probably makes the largest proportional part of its savings through life insurance rather than through the direct ownership of securities and property, including equities in businesses.

There is still further evidence of the importance of life insurance as a means of saving. According to a study of consumer savings in 1946, published in the *Federal Reserve Bulletin* for August, 1947, the payment of life insurance premiums constituted by far the most common form of individual savings. Out of every 100 spending units in the country that year, 76 saved through the payment of life insurance premiums, 18 saved through payments on mortgages, and 13 by payments into retirement funds. These figures are not mutually exclusive. The point I wish to make is that, as a means of popular saving, life insurance led the parade by a very wide margin, the number of spending units saving through life insurance being not greatly exceeded by the number of such units saving through all other media combined.

NATURE OF LIFE INSURANCE SAVINGS

Savings made through life insurance have certain noteworthy characteristics. First of all, they are relatively stable—much more so than the sum total of individual savings combined. The reasons for this can be readily surmised. As long as a life insurance policy is kept in force, the accumulation of a reserve becomes an automatic affair; and, in general, people tend to keep their life insurance in force even if they have to forego temporarily other types of savings. In brief, the institution of life insurance has tended to make the accumulation of monetary savings more of a continuous and automatic process than it would be otherwise.

Another characteristic of life insurance savings is the relatively circumscribed scope for their investment. By law and tradition, the investment of these has been limited largely to senior securities, which means very largely evidences of debt. In other words, their investment has been restricted very largely to bonds and real estate mortgages.

There are a number of reasons for this state of affairs. In the first place, life insurance assets have been traditionally regarded as trust funds and their investment in the most senior and secure investment media has seemed proper.

Second, the mathematical calculation of life insurance reserves assumes a fixed but certain rate of return, and it has seemed proper that

these reserves should be backed by assets which appear best able to assure this type of return.

Third, the above two considerations have been reinforced by law. The laws of most important states specify that life insurance funds, aside from certain limited proportions, should be invested in evidences of debt.

Fourth, regulations governing the valuation of life insurance assets tend to favor investment in bonds and mortgages. While well secured bonds and mortgages may be carried at cost or at amortized values, stocks, both preferred and common, must be carried at current market values. This tends to limit equity investments to a low percentage of assets because of the possible effect on the company's surplus of a serious decline in stock market prices. As a practical matter, therefore, life company investment in stocks rarely approaches the legally permitted limit, and seldom exceeds about 5 per cent of the assets of an individual company.

Because of the quite rigid manner in which the liabilities of a life insurance company are calculated, security of principal and reliability of rate of return are more important considerations than the chance for capital appreciation. Also, because of the manner in which life companies are taxed, tax exemption of income from securities is of very little value to them. In these respects, their position differs radically from that of wealthy individuals in high income tax brackets, to whom the chance to substitute a capital gains tax of 25 per cent for the regular income tax schedule is a prime consideration. The position of life companies with respect to tax exemption of securities is considerably different from that of the commercial banks, whose earnings are taxed on the same basis as those of corporations in general.

Because of these factors, life companies are mainly buyers of types of securities which have largely lost their appeal to individuals, namely, relatively high grade corporate bonds and mortgages. While there is some overlapping of interest between the life insurance companies and commercial banks, in general their requirements are different. Banks, to provide backing for their demand liabilities, invest largely in short term securities; while life companies, because of the steady growth in their assets and large and comparatively reliable cash inflow, do not in general feel a pressing need to sacrifice rate of return for liquidity. Also, tax exemption in a security has real value to a commercial bank, while in recent years the life companies have largely disposed of their tax exempt municipal bonds. However, because of sharply higher yields on municipal bonds, the life companies are again beginning to buy them on a small scale.

As a result of these factors and also because of the very large volume of funds at their disposal, the life companies have become the most important source of long term capital in this country of the type which is interested primarily in security of principal and sureness of rate of return. They have replaced the individual investor in this role since the last great period of capital expansion in this country—that of the twenties.

The relative importance of life insurance funds in the economy and the restriction, legal and otherwise, placed upon their investment, have given rise to a good deal of discussion and to some action, as we shall see later.

SUPPLY AND DEMAND FACTORS

For a long period in the history of this country, up to about a dozen years ago, the fact that life companies were largely restricted to investment in debt securities did not seem to create any serious problem. Up to the year 1930, at least, the growth in life company assets was approximately matched by that in long term private debt, consisting of corporate bonds and real estate mortgages. Up to that time, such private indebtedness had been the traditional channel of life insurance company investment, and in 1930 accounted for 84 per cent of the total invested assets of the companies.

But whereas in the decade of the twenties a $12 billion increase in life company assets was matched by a $30 billion increase in long term private debt, during the fifteen years beginning with 1931, life company assets increased $26 billion while private debt declined by $22 billion. Or, putting it a little differently, in 1930 life company assets equalled about one-fifth of the total long term private debt outstanding, while at the end of 1945 they were equal to nearly two-thirds of the total of such debt.

The point is that during this latter period, the traditional forms of investment media did not prove adequate to absorb the increase in life company funds. During this period, the life companies took over an increasingly large proportion of the total outstanding private debt. At the present time, for example, they own over one-half of the total bonds of the electric, telephone, and gas divisions of the public utility industry.

In the years just prior to the war, the life companies purchased a substantial volume of U. S. Government bonds, largely for want of other investment outlets; and during the war, their holdings of these bonds mounted to reach a total of nearly one-half of the total assets of the companies. During the war period, the yield on long term U. S.

Government bonds of 2½ per cent compared favorably with a yield of less than ¼ per cent more on high grade long term corporate bonds. This spread in yields has since about doubled, the yield basis on which long term high grade corporate bonds can be sold in quantity being at least ½ per cent higher than that on longest U. S. Government bond issues.

INVESTMENT IN REAL ESTATE

Probably the most noteworthy attempt to expand the investment field for life company funds was that into the direct ownership of real estate. Such real estate investments take two main forms—housing development and the ownership of business properties. The latter are usually leased to corporations of satisfactory credit standing on a basis to give the life company a moderate return and amortization of the investment over an extended period of years.

In recognition of the need for additional investment outlets, as well as the social desirability of permitting life company funds to flow directly into medium-cost housing (there is no low-cost housing any more), the laws of a considerable number of states have been amended to permit such investment. At the present time, the law of New York State, where life companies having about 60 per cent of the total assets of the industry are domiciled, permits investment of up to 10 per cent in housing projects built by the companies, and an additional 3 per cent in business or residential real estate. New Jersey, also an important state life insurance-wise, permits an investment of 5 per cent of assets in real estate held for business or residential purposes. Massachusetts currently permits 10 per cent of assets in rental housing and 10 per cent in other real estate. Connecticut and Wisconsin have catch-all provisions in their laws which permit investment in real estate or other investment media not otherwise authorized. Indiana has a law which permits investment in commercial real estate, subject to its lease to a corporation of defined credit standing, and in residential real estate.

THE PRESENT LARGE CAPITAL DEMAND

In the quite recent past, many people were fearful as to how far the lack of balance between supply and demand would go in depr ing yields available for fixed interest securities of the type in which life companies had traditionally been invested. Today, for the time being at least, such fears are somewhat abated. A good many of us failed to foresee the huge postwar demand for capital and the effect of inflation in stimulating that demand. The rise in prices has had the effect of re-

quiring from $1.50 to $2.00, or even more, to the work formerly done by $1.00.

Take the case of the utilities. From reading prospectuses, the impression is gained that many electric companies will make additions to their plants in the five years beginning with 1947 which will cost in dollars around 50 per cent of the cost of their existing plants. It costs about two-thirds more now than before the war to install a kilowatt of steam generating capacity.

American Telephone & Telegraph is currently engaged in a construction and financing program which will add about 40 per cent to its recent capitalization.

Many manufacturing and mercantile concerns have suddenly awakened to the fact that they need a lot of new money and need it rather badly—so badly that they are not in too good a position to argue about terms. It costs a lot more to carry inventory at present prices, and some inventories are unbalanced and a bit slow moving.

The recent demand for mortgage money, inflated by high real estate prices, has been particularly great; and some mortgages which were eagerly sought for a year ago now go abegging. Some life insurance companies have found themselves overly committed in their mortgage acquisitions and have practically withdrawn from that field.

Apparently, what we are witnessing is a sharp rise in the demand for investment capital, coupled with a continued decline in savings. High prices have probably reduced the ability of people to save. In any case, the interest on high grade long term bonds has increased by over ½ per cent in the last 1½ years, and the trend is upward. It currently appears that there will be a very large supply of such bonds sold for new money purposes over the next few years. Also, a yield of 3 per cent, which looked pretty bad to the institutional investor on the downside, now looks pretty good going up. With greener pastures available in their traditional investment fields, there is a greatly lessened incentive for life companies to seek new investment channels at this time.

FROM THE CAPITAL RAISER'S POINT OF VIEW

So far we have looked at this matter largely from the point of view of the life insurance investor. We will now attempt to look at it briefly from the point of view of the party requiring capital. Take the public utility industry as an example. It is perhaps the prime example of an industry requiring a large amount of funds of the type supplied by the life companies, that require a reasonably steady rate of return with capital appreciation as a quite secondary consideration.

It has come to be regarded as proper by the regulatory authorities that a utility should raise not much over half of its required capital in the form of debt and the balance by the sale of equity securities—preferred and common stocks—or by retained earnings. As long as they maintain their credit position, the utilities can probably raise capital by the sale of these bonds to life insurance companies. But what about balancing the sale of these bonds by that of equity securities on which, among other things, the maintenance of this credit position depends? During the last three years, the life companies have bought a rather large volume of utility and other preferred stocks. Recently, however, they have turned extremely cold toward this class of security, partly because some life companies have about reached their limit in this field, due to the reasons previously outlined, and partly because of the quite bad recent market behavior of these securities. Life companies have never been substantial buyers of utility common stocks.

Because of this situation it seems likely that the utilities are going to have trouble balancing their bond sales with stock sales unless they are prepared to do their stock financing on a relatively high cost basis. In addition, recent events have proved that they must also increase the attractiveness of their preferred stock financing by the insertion of sinking funds or convertibility features. They may even be forced to substitute convertible debentures for equity securities in their capital structures as far as the raising of new money is concerned. The substantial withdrawal of the life companies from the preferred stock market has made clear the extent to which they have rather recently come to dominate that market. Their dominance of the corporate bond market has been clear for at least a decade.

The sale of bonds has not been the traditional way for most branches of American industry to raise capital. In the past, only a few branches of our industry—the most important of which are steel, rubber, meat packing, and tobacco—have raised any appreciable part of their capital through the sale of bonds. On the other hand, some leading divisions of industry—including chemicals, electrical manufacturing, and many others—have been financed largely through equities. A good many types of industry, because of the relative irregularity of their earnings, would probably be well advised to avoid the use of contractual obligations for raising capital, if at all possible. However, the relative ease with which the funds of life companies can be tapped through the debt route, compared with the very great difficulty of tapping them through any other route, may put pressure on some industries to sell bonds when preferred or common stocks would be more suitable.

The emergence of the life companies as the main suppliers of fixed return capital in this country has had a major influence on the underwriting and distribution of securities. The selling of securities in large blocks to life companies, plus competitive bidding, have caused new issues to be handled on very small gross profit margins to underwriters, which would have been impossibly small if sale and distribution had to be made to a host of individual buyers. In view of the current attitude of the life companies toward preferred stocks, these may again have to be sold to individuals; the gross profit to underwriters at which they are handled will have to widen materially. The coupon rates also will have to be increased materially above recent levels to be attractive to individuals in substantial income tax brackets.

It is a well-known fact that keen rivalry exists among the large life insurance companies in the making of investments. They make their decisions on a highly individualistic basis and frequently pursue divergent investment policies. This, however, does not obviate the fact that under present conditions, success or failure in the underwriting of corporate bond issues of any size depends largely upon the willingness of at least some of the largest life companies to buy substantial amounts. Not only does this serve directly to get the bonds sold, but it no doubt influences the action of smaller investing institutions.

The dominance of the corporate bond market by the life companies has, of course, led to the private deal. It is probably natural that if a group of life companies are going to take most of an issue anyway, they should band together to shape the terms as much as possible to their liking, and as a secondary consideration save underwriters' profits. From the point of view of investing institutions, the advantages of such deals are that stronger protective terms can be imposed than in corresponding public issues, and a little better yield obtained.

From the point of view of the borrower, advantages include the avoidance of SEC registration, the chance to deal with only a limited number of professional investors in case a change in the terms is later desired, and the certainty of sale. Often an investment banker acts as agent in presenting the deal. In the case of a small issue of securities, particularly if put out by a borrower of secondary or unestablished credit standing, the saving to the borrower of going the private route may be very great indeed.

MORTALITY STATISTICS FOR LIFE INSURANCE UNDERWRITING*

EDWARD A. LEW
Metropolitan Life Insurance Company

The paper deals chiefly with those mortality statistics, compiled from the experience of insured persons, that have been used as a guide in life insurance underwriting. Properly interpreted, these statistics shed considerable light on the factors influencing human longevity. The use made of other mortality statistics in the underwriting of life insurance risks is also indicated.

LIFE insurance companies have conducted many investigations of mortality to obtain the statistics needed for the calculation of premiums, as a guide in the selection of risks, and for other purposes. Aside from serving their primary purposes, these studies contain a wealth of information regarding the factors influencing human longevity. In order to interpret the results intelligently, it is necessary to know something about the particular purposes of the various studies, the methods used in the compilation of the statistics, and their limitations.

This paper is confined to a consideration of the mortality statistics for life insurance underwriting. The term "life insurance underwriting" is applied to the procedures used by life insurance companies in passing upon applicants for life insurance. Broadly speaking, the objective of these procedures is to appraise applicants' prospects of longevity so as to classify them either into the various acceptable categories that have been established or the type which should be rejected, and to determine the appropriate premium rates for applicants in the acceptable categories.

The aims of this paper are:

(1) To sketch briefly how mortality studies for underwriting began in this country,
(2) To indicate the extent to which underwriting considerations have shaped both the form and content of the mortality studies for life insurance underwriting,
(3) To review the methods used in the more important investigations made by life insurance companies to obtain information for underwriting purposes,

* A paper presented at the 107th Annual Meeting of the American Statistical Association, New York City, December 30, 1947.

(4) To consider the principal findings of these investigations in the light of the limitations of the data, and
(5) To examine the usefulness for underwriting purposes of mortality statistics from sources other than the experience of insured lives.

ORIGIN OF MORTALITY STUDIES FOR UNDERWRITING

The efficacy of life insurance underwriting depends largely on knowledge regarding the effects on mortality of those characteristics of life insurance applicants which can readily be ascertained in practice. That detailed quantitative knowledge is not absolutely necessary is indicated by the earlier successful record of the life insurance business, first in Great Britain and later on this continent. Even in the first part of the eighteenth century, there was sufficient qualitative understanding of many factors affecting human longevity, based on common sense observations, to have led some English companies to charge extra premiums to travelers going to Ireland, to women of child-bearing age, and to persons who had not had smallpox. Generally speaking, however, life insurance underwriting before the advent of mortality statistics was primarily concerned with eliminating applicants who were in poor health or otherwise undesirable risks on account of intemperate habits, questionable morals, etc.

More exact knowledge of mortality, based on statistical investigations, had to wait until sufficient data had been accumulated and suitable methods for extracting the information devised. That the data became available as soon as they did is in no small measure due to the remarkable growth of life insurance in the United States. With increasing numbers of persons rejected for life insurance during the last decades of the nineteenth century, there arose in the course of time an effective demand for the insurance of substandard risks. American companies sought to make such insurance available and turned to their actuaries to devise methods and to determine premium rates for insuring substandard risks. In 1889 the Actuarial Society of America was organized and this body naturally brought to bear on these problems the techniques originated and the experience with mortality investigations gained by actuaries in Great Britain. No less opportune was the introduction at about this time of punch card equipment for making mortality investigations. As a result of these developments, and because of the accumulation of a sizeable volume of data by reason of the rapid growth of life insurance, the Actuarial Society of America was able by 1901 to undertake an extensive intercompany investigation (Specialized Mortality Investigation) of various factors affecting the mortality of

insured lives. This investigation was the forerunner of many similar efforts, both intercompany and by individual companies, designed to provide mortality statistics for life insurance underwriting.

MORTALITY STATISTICS ADAPTED TO UNDERWRITING CONSIDERATIONS

Mortality statistics derived from the experience among insured lives furnish the most pertinent guide for life insurance underwriting. This is because they show the actual results obtained for the type of information normally elicited in life insurance applications and from other sources available to life insurance companies. While our general knowledge of the factors influencing mortality has been greatly enriched by the increasing numbers of statistical studies made by research groups, hospitals, physicians, and others, such studies have sometimes only limited applicability to life insurance underwriting.

Mortality factors considered in underwriting. Most of the mortality investigations made by life insurance companies to obtain information for underwriting purposes have been concerned with those particular characteristics known to affect mortality about which information could readily be obtained by an insurance company. In addition to age, the principal characteristics so used in appraising applicants' prospects of longevity have been: (1) occupation, (2) height and weight, (3) physical impairments brought to light by the usual type of medical examination for life insurance, (4) personal medical history and family history as disclosed in response to questions asked in life insurance applications, and (5) habits as reported by independent inspections. These are by no means the only characteristics highly correlated with mortality variations or even necessarily the most important ones, but over the years it has been found possible to secure satisfactory information about them through the channels available to life insurance companies. Details of these characteristics are obtainable more accurately on applications for Ordinary insurance than on other forms of life insurance and as a result studies of mortality made for underwriting purposes have been predominantly based on the experience of lives insured under Ordinary policies.

Numerical rating system. Gradations in the factors commonly considered in underwriting are expressed in terms of the excess mortality they are expected to bring about among lives otherwise acceptable for insurance at standard premium rates. This excess mortality for a group having a specific characteristic, stated as a percentage of the mortality among a similarly constituted group of lives acceptable for standard insurance, is referred to as the rating for the characteristic; by adding the

ratings for various characteristics of a risk, treating interrelated characteristics as a single factor, a numerical appraisal of the total risk is obtained. This method of appraising longevity in numerical terms (known as the numerical rating system) requires that the ratings for the various characteristics be based on actual experience whenever possible.

Mortality ratios. To facilitate interpretation of mortality statistics as ratings, mortality data for insured lives have customarily been summarized as ratios of the deaths actually experienced in a group to the deaths that would have occurred had the group been composed of otherwise comparable lives acceptable at standard premium rates. Such ratios show only the *relative mortality* associated with a particular characteristic during a given period of observation, but this is the criterion usually employed in classifying risks for underwriting purposes. It should be noted that in determining both the numerator and the denominator of these ratios, the number of policies terminated by death (or the amounts of claims paid) has generally been used in lieu of the number of persons involved. While the mortality ratios by number of policies (or amounts of insurance) may occasionally exhibit extraneous fluctuations due to a large number of policies (or a substantial amount of insurance) being involved in a single death, they have the advantage of reflecting more accurately the financial effect of the extra mortality traceable to particular characteristics.

Risks appraised as members of a class rather than as individuals. A very precise appraisal of various characteristics in numerical terms is neither practicable nor necessary for classifying applicants as life insurance risks. Only on very large amounts of insurance does it become worth while to make elaborate investigations. For most practical purposes, it is sufficient to be able to make decisions as to the broad class of risk to which a particular applicant should be assigned. The averaging of risks is a fundamental principle of insurance and the underwriter accordingly looks upon risks more nearly as members of a class than as individuals. In line with this principle, ratings are more appropriately regarded as mean values of a reasonable range of variation. In making mortality studies of various characteristics for the purpose of determining ratings, actuaries have, therefore, often preferred to deal with risks in broad groups, whose heterogeneity corresponded to that of the rating classifications, rather than subdivide the experience into more homogeneous but smaller groupings subject to large chance fluctuations.

Risks classified on basis of characteristics at time of application. Life

insurance policies do not as a rule contain any provisions which enable the company to change the classification of a risk after issue, even though the risk increased materially. Applicants for life insurance must, therefore, be appraised on the basis of their characteristics at time of application for the insurance, taking into consideration the possibility of the risk's increasing. This has important implications as to the type of mortality statistics needed for life insurance underwriting. For characteristics that can be changed by the insured at his own volition, such as occupation or aviation activities, the ratings must be determined from an experience of closed groups of individuals having the particular characteristics at time of entry; such experience should reflect the effect of the usual proportion of individuals changing to a more hazardous status as well as the proportion of individuals who change to a less hazardous status and leave the group. For characteristics where the excess mortality does not manifest itself until many years have elapsed, such as overweight or moderate elevation in blood pressure, the ratings must be determined from experience of sufficiently long duration.

Incidence of mortality by age and duration. Because premiums for life insurance depend to a marked degree on the incidence of mortality by age and duration from issue, mortality statistics for life insurance underwriting have usually been compiled so as to bring out the variations in mortality by age and duration. Such information is not only helpful in appraising and classifying risks but indicates also the type of extra premium to be charged—constant with age, increasing with age, decreasing with duration, etc. Mortality statistics by number of policies and especially by amounts of insurance are, of course, more suitable than mortality statistics by lives for the purpose of determining extra premium rates.

METHODS USED IN MORTALITY INVESTIGATIONS OF INSURED LIVES

Intercompany mortality investigations. The principal investigations made to obtain mortality statistics for underwriting purposes have been carried out by the Joint Committee on Mortality of the Actuarial Society of America and the Association of Life Insurance Medical Directors. The first study made by this Committee (Medico-Actuarial Mortality Investigation) was based upon the combined experience of a number of companies under Ordinary policies issued during the years 1885–1908 and included average heights and weights, the effects of build on mortality, the mortality of women and certain races, mortality by occupation and mortality for various medical impairments. Since that time, nine additional mortality investigations have been made by

the Joint Committee, covering chiefly medical impairments and occupations.[1]

These later investigations have all dealt with the combined experience among lives insured under Ordinary policies in the larger life insurance companies of the United States and Canada. The usual procedure has been for individual companies to send to the Joint Committee summary sheets for each class of risk included in the study showing (1) the "exposed to risk," (2) the "expected" deaths, and (3) the actual deaths, tabulated by broad age groups at issue and appropriate groupings by duration from issue. As the basis for calculating the "expected" deaths in each investigation, the Joint Committee has constructed special mortality tables (often referred to as "basic tables") in "select" form, i.e., showing mortality rates by age groups at issue and duration from issue. These tables represent the companies' combined experience under Ordinary life insurance issued at standard premium rates for the period of years covered by the investigation. Special tables have also been prepared to serve as yardsticks of the expected mortality from the major causes of death.

The data for these mortality investigations have been assembled by the individual companies on punch cards, prepared in the larger companies for a sample of the Ordinary policies issued at standard premium rates and for all Ordinary policies issued at substandard premium rates. The punch cards have been coded for occupation, medical impairments, and certain other characteristics in accordance with codes specially prepared by the Joint Committee on Mortality. Occasionally, it has been necessary to go back to application papers in order to study characteristics which were not distinguished in the codes published by the Joint Committee.

From time to time, the Joint Committee has asked the companies for suggestions as to the classes of risks whose mortality it would be advisable to investigate. The classes favored have then been reviewed to determine whether the volume of data was large enough to warrant their inclusion in the study. For this purpose the larger companies have maintained up-to-date tabulations of the number of deaths on policies with rateable occupations, medical impairments, etc.

As a general rule, in selecting cases with a particular characteristic for inclusion in a study, it has been customary to eliminate those that

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[1] Blood Pressure Study 1925; Joint Occupation Study 1928; Medical Impairment Study 1929; Supplement to Medical Impairment Study 1932; Impairment Study 1936; Occupational Study 1937; Impairment Study 1938; Blood Pressure Study 1939; Supplement to Blood Pressure Study 1939.

also presented other characteristics which by themselves would have precluded the issuance of a policy at standard rates. On the other hand, cases accepted at standard rates have been included even though they might exhibit minor characteristics not of sufficient importance to require an extra premium but nevertheless of possible significance. Under these circumstances, the experience of a class may to a degree reflect the excess mortality attributable to the cases with minor characteristics. The proportion of cases with such minor characteristics has usually been a factor in determining whether a particular class should be studied, and in some investigations the cases with minor characteristics have been eliminated to obtain an experience more truly representative of the characteristic under study.

Shortcomings of intercompany studies. There has been some question as to whether mortality investigations of the combined experience of many companies with different underwriting practices and standards yield significant results for certain classes of risks. The point has been made that the mortality experience of companies having similar underwriting practices and standards could be interpreted more accurately and that the best guide for an individual company was its own experience. Because of different application forms, different medical examination requirements, and different attitudes toward indefinite reports, it has been found that the cases included by different companies in classifications which are not susceptible to a precise definition may differ markedly. However, the data included in classifications that can be clearly defined have been found to give a satisfactory degree of homogeneity. Present opinion favors a continuation of mortality investigations of the combined experience of many companies for classes where the data are homogeneous or where a sufficiently large experience could not be obtained otherwise.

Individual company and other mortality studies of insured lives. Studies of the experience of individual companies have also contributed much information regarding effects of various characteristics on mortality.[2] The results have generally been easier to interpret than those of intercompany investigations because of greater homogeneity of the data and the greater ease with which questionable points could be reviewed. On the other hand, small numbers have been more frequently encountered in individual company studies, so that recourse has sometimes been

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[2] Among the more notable of these have been a series of papers by A. Hunter and O. Rogers (*Transactions* of the Actuarial Society, Vols. XXIII to XXX) and another series by L. I. Dublin in collaboration with various others (*Proceedings* of the Association of Life Insurance Medical Directors, Vols. XV, XIX to XXVI, and XXVIII).

had to more advanced statistical techniques to gauge the reliability of the results. While individual companies' mortality investigations have as a rule been made from the same punch cards as those used in intercompany investigations, the methods employed have, of course, varied considerably. For instance, some companies have occasionally determined their experience for various classes of risks by relating all the deaths in a class to exposures built up from a sample of the policies issued.[3]

The techniques used in follow-up studies of individuals subjected to special examinations and of rejected risks also deserve attention because of their applicability to other fields. The usual practice has been to trace individuals chiefly by reference to their insurance holdings in various companies. It now appears possible that Social Security records might be utilized for this purpose, if suitable arrangements can be worked out with the Social Security Administration, and the records are handled in such a manner that particular individuals cannot be identified.

FINDINGS OF THE PRINCIPAL MORTALITY STUDIES OF INSURED LIVES

In looking at the findings of the principal mortality studies of insured lives, it is necessary to keep in mind that they represent the experience among persons insured under Ordinary policies. Such persons comprise a highly selected portion of the general population; they are predominantly white male adults, and in the past at least were drawn chiefly from the more prosperous classes of the population. Even when insured at substandard premium rates, they usually do not present characteristics that result in a drastic reduction in longevity.

While absolute death rates for various classes of risks can be derived from the mortality ratios developed in these investigations, the mortality ratios themselves indicate only the relative mortality of different classes of risks, compared with the level of mortality among lives insured at standard premium rates during the same period of years. Since the level of mortality among lives insured at standard rates has been changing over the years, different yardsticks of mortality have in effect been used in different studies. As the level of standard mortality has declined, the relative mortality of risks whose absolute mortality rates remained unchanged has increased, while the relative mortality

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[3] See paper "Sampling Errors in Life Insurance Mortality and Other Statistics" by Donald Cody to be published in the September 1948 issue of the *Journal of the American Statistical Association*.

of risks whose absolute mortality rates decreased more rapidly than that of standard lives has declined.

Most of the mortality investigations of insured lives represent experiences spanning relatively short periods of time. The great volume of the experience has usually been in the years immediately following issue of the insurance, while the experience at the longer durations has sometimes been too scant to give reliable information. This is a serious shortcoming in the case of those classes of risks where the extra mortality is deferred, since the results will tend to understate the excess mortality that may actually be experienced over the entire period of insurance.

Despite these and other limitations, mortality studies of insured lives provide much pertinent information of a broad qualitative nature as to the effects on mortality of occupations, height and weight, physical impairments, and medical histories. In interpreting the experience of particular groups of lives, it is essential to have a clear idea as to the type of cases included in each classification. These points are illustrated in the following discussion of the findings of the more important studies.

Occupations. The first extensive intercompany investigation (Specialized Mortality Investigation) covered the experience during the years 1870–1900. It included a study of 35 groups involving an occupational hazard, among them commissioned officers, the more important hazardous trades, liquor distribution, and railroad service.

The Medico-Actuarial Mortality Investigation, which covered the years 1885–1909, included 168 groups of occupations. The most important of these were in the liquor industry, railroad service, metal trades, and mining. This study showed that mortality was particularly high among underground miners, railroad engineers and firemen, structural iron workers, stone cutters, and many occupations in the liquor industry. In certain hazardous occupations, the unusually low mortality experienced was believed to be due to very careful selection of risks by the companies and to the short average duration of the experience.

The most comprehensive investigation of occupations thus far conducted was the Joint Occupation Study. It called for the study of some 430 groups of occupations and covered the years 1920–27 for occupations with a major accident hazard and the period 1915–27 for other occupations. The occupations studied ran practically the entire gamut of occupational hazards commonly met with among applicants for Ordinary life insurance. Very high mortality was found among metal miners, coal miners, oil and gas field rig builders, linemen in electric

light and power plants, chippers of metal, laborers in iron and steel works and foundries, and bartenders. Tuberculosis was found to be high in some 25 occupational classes, chiefly among unskilled labor and in occupations characteristic of the lower income groups, and also among those exposed to the silica dust hazard, notably stone cutters. A special study was also made of the accidental death rates, differentiating between those due to occupational and non-occupational accidents. The highest accidental death rates were found among electric light and power linemen, oil and gas field rig builders, underground miners, structural iron workers and freight train brakemen.

The most recent of the investigations dealing with occupational hazards—the Occupational Study of 1937—covered the years 1925–36 and included 87 occupational groups. Among them were many for which the results in the Joint Occupation Study had been inconclusive. Very high mortality was found among metal miners (high accidental death rate, tuberculosis and pneumonia), motorcycle police (high accidental death rate), anthracite coal miners (high accidental death rate, tuberculosis and pneumonia), chippers of metal (high tuberculosis), electric light and power linemen (high accidental death rate), and stone cutters (high tuberculosis). The study showed a decrease in the mortality of laborers in non-hazardous industries, believed to reflect in some measure the improvement in the living conditions of the lower income groups.

A review of the findings of these investigations indicates that the mortality in occupations with pronounced accidental death or health hazards has in general increased relative to the mortality of lives insured under Ordinary policies at standard premium rates; on the other hand, the relative mortality in many occupations where the excess mortality was at one time largely due to low living standards appears to have decreased. This supports the significant conclusion reached from various studies that the effects of occupation on mortality are, on the whole, more indirect than direct; mortality in most occupations is influenced more by the environment and the living standards associated with the occupation than by the accident and health hazards traceable to particular occupational duties.

Height and Weight. The first table of average weights according to height and age, based on a considerable volume of statistics, was prepared from life insurance data by the Association of Life Insurance Medical Directors in 1897 and was known as the Shepherd table.

The Medico-Actuarial Mortality Investigation developed new tables

of average heights and weights by age for men and women and also included an extensive investigation of the effects of build on mortality. The height and weight tables were based on the records for 221,819 males and for 136,504 females, representing a random sample of the heights and weights, in street clothing (without overcoats), of persons to whom standard Ordinary policies were issued mainly during the years 1885 to 1900, inclusive. These tables of average heights and weights remain in general use today, because subsequent studies showed that there has been little change in average heights and weights over the years. The investigation of the effects of build on mortality brought out the following main conclusions:

(1) That mortality increases with decreasing weight at the younger ages,
(2) That the effect of underweight on mortality decreases with advancing age,
(3) That overweight is not a serious impairment at the younger ages but its adverse effect on mortality increases with advancing age,
(4) That the lowest mortality at ages under 35 is found among those 15 to 20 pounds above average but at ages 35–49 it occurs among those 5 to 10 pounds underweight,
(5) That young underweights have high mortality from tuberculosis and pneumonia,
(6) That overweights have high death rates from diabetes, nephritis, heart disease, and cerebral hemorrhage.

Average heights and weights were determined again in the Medical Impairment Study for persons to whom standard Ordinary policies were issued during the years 1909–27, but no new tables were published for ages 25 and over because the figures developed were found to be remarkably close to those established by the Medico-Actuarial Mortality Investigation. New tables of average heights and weights were, however, compiled for ages 10 to 24. The Medical Impairment Study also confirmed the major findings of the Medico-Actuarial Mortality Investigation in regard to the effect of build on mortality.

It appears likely that the relative mortality of both overweights and underweights has changed somewhat since the publication of the 1929 Medical Impairment Study. This study did not extend over the recent period of years during which mortality from tuberculosis and pneumonia showed a marked decline. Since these causes of death have in the past accounted for practically all of the excess mortality among underweights, it would be reasonable to suppose that the mortality of underweights has improved in relation to that of lives insured under Ordinary

policies at standard premium rates. On the other hand, there has apparently been little change in the mortality from heart disease, diabetes, and other degenerative diseases, which have accounted for most of the excess mortality among overweights. This suggests that the relative mortality of overweights may have increased.

Physical Impairments and Medical Histories. The particulars given in life insurance applications concerning occupation and height and weight can as a rule be taken without qualification. However, this is not the case in much of the information elicited regarding physical impairments and medical histories.

The medical examination for life insurance consists usually of two parts. The first is made up of the applicant's own statements regarding details of his physical condition, history of illnesses, injuries, and medical treatment received, as well as some miscellaneous matters such as family history. The second part is in the nature of a report by the medical examiner on the physical examination of the applicant; this examination customarily embraces a check on the important organs of the body, height and weight, a blood pressure reading and urinalysis. Special tests such as an electrocardiogram, X-rays, and blood sugar tests are expensive and can be used only on large amounts of insurance or in other cases where they are clearly of great value. There are also practical limits on the extent to which indefinite statements made by the applicant or the medical examiner can be followed up. The result is that many of the descriptions of physical impairments and medical histories on life insurance applications represent relatively undifferentiated conditions. The findings of the physical examination itself cannot usually be compared in point of comprehensiveness and precision with those made by clinicians who are in position to study a patient for a longer period of time, more intensively and with his full cooperation. As a consequence, the mortality experienced among insured lives with certain physical impairments or medical histories furnishes a guide of only limited relevance for cases where specific and accurate diagnoses have been established.

Because persons suffering from serious organic diseases or from acute illnesses would not ordinarily apply for or be accepted for life insurance, the mortality studies of insured lives have perforce dealt with the less severe and non-acute conditions. With few exceptions, clinical medicine has not been able to follow large numbers of patients with relatively minor impairments for a sufficiently long period of time to determine their effect on mortality. Despite their limitations, mortality studies

of insured lives constitute, therefore, a major source of information regarding the significance of many such impairments. The most valuable contributions to medical science made by mortality studies of insured lives have probably been with respect to factual conditions that can be appraised in numerical terms, such as build, blood pressure, urinalysis findings and pulse rate.

The earliest of the intercompany studies—the Specialized Mortality Investigation—included 24 groups of physical impairments and medical histories. However, only cases accepted at standard premium rates were included and some of the classes were described in terms no longer current. The Medico-Actuarial Mortality Investigation included 83 groups of physical impairments and medical histories, all but two of which were limited to cases accepted at standard premium rates. The list of impairments studied was not greatly different from that of conditions considered significant today. Among the many results of interest, it was found that persons with a rapid pulse rate of between 90 and 100 per minute showed substantially higher than normal mortality, while those with a pulse rate over 100 per minute experienced even higher mortality. Later studies corroborated these findings.

The most comprehensive of the intercompany investigations of physical impairments and medical histories was the Medical Impairment Study. It covered the experience on policies issued during the years 1909–27 and included 122 groups of impairments and histories, many being further subclassified by duration and severity. Cases accepted at extra premium rates as well as those accepted at standard premium rates were studied. Because of the inclusion of cases accepted at extra premium rates, the mortality associated with a number of more serious conditions was developed for the first time in an intercompany study.

Probably most important among the serious conditions were several classes of heart impairments, diagnosed primarily by reference to types of heart murmurs they produce. The mortality experienced on certain of the heart murmurs confirmed clinical opinion to the effect that they were relatively unimportant, but other types of murmurs were found to be associated with very high mortality. A heart murmur in conjunction with a moderate degree of hypertrophy of the heart and particularly with a history of rheumatic fever was shown to be a very serious condition. Heavy manual work was found to have an adverse effect on the mortality of those with heart disease.

The Medical Impairment Study included also the experience for other combinations of impairments such as heart murmurs with elevated blood pressure, albumin with casts, and albumin with elevated

blood pressure. It was found that the combination of albumin and overweight presented a very much more serious impairment than the sum of the extra mortality associated with each of these conditions would indicate.

The Medical Impairment Study brought out clearly for the first time that even small elevations in systolic blood pressure are associated with a marked increase in mortality. This finding was at variance with general medical opinion and a number of additional investigations were made to learn more about the effects on mortality of variations in blood pressure. The most important of these was the 1939 Blood Pressure Study, which considered the variations in both systolic and diastolic pressures. The main conclusions of the several studies of blood pressure were as follows:

(1) A systolic blood pressure of 20 mm. above average or a diastolic blood pressure of 15 mm. above average results in distinctly higher mortality than normal.
(2) Mortality is heavier where both systolic and diastolic pressures are high.
(3) The combination of elevated blood pressure with certain minor impairments, particularly those of the cardiovascular-renal system, produces appreciably higher mortality than the sum of the extra mortality associated with the respective conditions.
(4) The lowest mortality is found in the group with systolic pressures from 5 to 15 mm. below average.

Two additional mortality studies supplemental to the Medical Impairment Study were made in 1936 and 1938. They covered 42 classes of physical impairments and medical histories. Further evidence was adduced by these studies as to the adverse effect on mortality of gastric and duodenal ulcers, removal and drainage of gall bladder, toxic goitre, syphilis, renal colic, irregular and intermittent pulse and albuminuria. The findings for several of these classes have probably lost some of their significance because of the recent advances in medical treatment and in surgery; the relative mortality of conditions that may require surgery, such as gastric and duodenal ulcers, is likely to have declined somewhat and the same probably applies to the mortality associated with syphilis.

STATISTICS FROM SOURCES OTHER THAN THE EXPERIENCE OF INSURED LIVES

It usually takes a long time to accumulate sufficient data for a mortality investigation of insured lives with relatively rare characteristics. For many types of risks it is virtually impossible to obtain mortality

statistics based on the experience of insured lives. In such cases the underwriter naturally falls back on whatever other information is available. Another difficulty is that by the time the mortality statistics have been compiled, the relative hazard of a characteristic may have changed materially. This has occurred in the case of certain occupations where the working and living conditions have improved markedly in recent times, probably also to some extent for overweights and underweights because of the changing mortality trends, and for a few medical conditions where new and radical methods of treatment have been discovered. Since the underwriter is primarily concerned with the mortality likely to be experienced on various types of risks in the future, he must keep in close touch with changing conditions in industry, with developments in medicine and public health, and with mortality trends in general. The availability of more and better public health statistics, medical statistics, and general population statistics has been of great help to actuaries and underwriters in making judgments regarding the probable future mortality of different classes of risks.

Life insurance companies have made considerable use of the records available in governmental agencies and in industry to estimate death rates for many occupations. The statistics maintained by governmental agencies have made it possible to determine the mortality rates among personnel of the armed forces, scheduled airline pilots and state police, to mention only a few. It has been more difficult to derive death rates for various occupations in industry, because records for individual occupations are rarely segregated and because often only the deaths due to occupational accidents can be traced. Even this limited information, however, has been of value in considering occupations where accidents are the major hazard. It might be noted that appraisals of occupational hazards based on government or industry statistics represent usually the death rates among those actively at work. For underwriting purposes, it may be necessary to make allowances for the higher mortality among those in the occupation who have become disabled and left work and for the proportion of the individuals who change to more hazardous occupations in the normal course of events.

Statistics compiled by hospitals, physicians and public health agencies have also been widely consulted for life insurance underwriting. Clinical statistics have been helpful in determining appropriate ratings for the many relatively rare conditions on which life insurance experience can shed no light. Such data have also proved of value in indicating under what circumstances a relatively serious impairment, such as diabetes, previously considered uninsurable, may be accepted at extra

premium rates. Although public health and population statistics have served chiefly to bring out changes in mortality trends, they too have occasionally been used as a basis for ratings. The application of clinical data to medical underwriting has presented many difficulties: for instance, the problem of translating the findings of accurate clinical diagnoses into the less definite categories found on life applications, and the difficulty of allowing for the difference between the details normally disclosed by a patient to his physician and the details stated on an application for life insurance, which may refer to conditions that have never received medical attention. It has also been necessary to make allowance for the relatively short term outlook in clinical medicine in contrast to the long term view of life insurance underwriting. The increasing use of follow-up studies by research groups, hospitals, and physicians indicates a growing appreciation of the need to study the long range effects on mortality of various medical conditions.

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STATISTICAL PROBLEMS IN DEVELOPING VOLUNTARY MEDICAL EXPENSE INSURANCE PLANS*

MORTON D. MILLER

The Equitable Life Assurance Society of the United States

Of the 45,000,000 persons having hospitalization insurance, 20,000,000 have become covered by some form of insurance for the expenses of medical care. Most of this coverage has been provided since 1938, principally by the life insurance companies through group insurance. This paper discusses the statistical problems accompanying the development of medical expense insurance plans: the sources of data, the difficulties encountered in adapting the data to the purpose, and the nature and purpose of the statistics to be collected as experience unfolds. Insurance company experience with medical expense insurance is producing much valuable information regarding the demand for various medical services, the cost of such services and the administrative problems involved in providing for medical services on a prepayment basis.

THE TREMENDOUS growth of hospitalization insurance is a fairly familiar story. In the relatively short period since 1930, the number of persons insured has risen from almost nothing to the point where today more than 45,000,000 persons are covered for the cost of hospitalization. The development and spread of voluntary plans to insure the cost of doctors' services promises to be equally phenomenal.

It is only since 1938 that the established insurance companies have been insuring surgeons' charges for the performance of surgical operations and obstetrical procedures, and since 1943 that the companies have been covering the cost of other types of medical services. Now in 1947, over 20,000,000 persons are insured for surgical benefits, of whom more than $\frac{1}{3}$ are covered in addition by some form of medical expense insurance for non-surgical services.

While community organizations similar to and sometimes affiliated with the Blue Cross hospital service associations have participated in the development of medical expense insurance, the established insurance companies have taken the lead in promoting these new forms of protection. The companies insure the majority of the persons covered, principally through group insurance written by the life insurance companies. Group insurance, as distinguished from individual insurance, is provided by means of a single master contract issued usually to an employer to insure the group of his employees and frequently their wives and children.

* A paper presented at the 107th Annual Meeting of the American Statistical Association, New York City, December 30, 1947.

Group surgical insurance provides cash reimbursement for the amount of the surgical or obstetrical fee, subject to a maximum amount of reimbursement which is dependent on the nature of the operation. The maximum amounts of reimbursement with respect to various types of operations are set forth in a schedule that appears in the master group policy and in the certificates of coverage furnished to the individual employees. In those cases where the surgeon's charge is in excess of the maximum amount of reimbursement for the operation, the insured employee receives the maximum reimbursement from the insurance company and pays the excess from his own funds. Coverage of obstetrical procedures is optional but is usually included.

A typical schedule of maximum reimbursement will be found to provide a maximum reimbursement of $150 for the most serious operations, such as cutting into the cranial cavity, maximum reimbursements of $100 for an appendectomy, $50 for an operation for a single hernia, $25 for a tonsillectomy, and $50 as the obstetrical benefit for a natural delivery.

Although group surgical insurance is a form of group medical expense insurance, the term "group medical expense insurance" has come to be used to describe the plans of group insurance covering the costs of medical services for other than surgery and obstetrics. A wide variety of such plans with differing benefits is available. Reimbursement may be provided for doctors' visits, usually in the amount of $2 for visits by the patient to the doctor's office or by the doctor at a hospital, and in the amount of $3 for visits by the doctor to the patient's home. Coverage may be comprehensive and include reimbursement for the expense of all visits; sometimes the first one or two visits in any illness are excluded. More limited coverage plans are also being written. Under total disability plans, reimbursement is made only for the doctor's visits needed in illnesses or injuries which are disabling; under in-hospital plans, only those visits in medical cases requiring hospitalization are reimbursed. The coverages for doctors' visits may be supplemented by additional coverage for the cost of diagnostic x-ray and laboratory examinations, in accordance with a schedule of maximum reimbursement for such examinations analogous to the schedule of maximum reimbursement for surgical operations.

These new forms of insurance involve numerous insurance and statistical problems. This is equally true whether the coverage is being written by an established insurance company or by a community plan, similar to the Blue Cross, although sometimes the sponsors of such plans do not appreciate this fact as fully as they might.

The first problem is that of securing some basis for the establishment of the initial premium rates. The basic factors required for the calculation of premium rates are: first, a rate or frequency of occurrence of the event insured against, for example, the annual rate of surgical procedures per hundred insured; and, secondly, the average amount of the benefit payable when the event occurs.

While there were no insurance data directly applicable to the calculation of premium rates for group surgical insurance, experience developed in connection with hospitalization and other forms of insurance did furnish collateral information that was helpful. One company computed a scale of premium rates for group surgical insurance as follows.

At first, surgical coverage was limited to those cases where the surgery was performed in a hospital. This made it possible to base the annual frequency of surgical procedures on the annual rate of hospitalization, which experience showed to be 7 per hundred for male employees. It was observed that $\frac{3}{4}$ of all hospitalized cases involved surgery and accordingly the annual frequency of surgical procedures for male employees became 5.3 per hundred. Then, after study of the proposed schedule of benefits, it was estimated that $50 would be the average amount of each claim. The product of the frequency and the average benefit gave $2.65 for the net annual claim cost for a male employee. This figure, when increased by the amount calculated to be necessary for expenses of operation and contingencies, resulted in a gross monthly premium rate of $.35 per male employee.

The gross monthly premium rate for a female employee was set at $.70, double that for a male employee. This was consistent with the experience under other forms of disability insurance, including both weekly indemnity benefits and hospitalization insurance, where the claim cost of employed females was observed to be twice that of males. In practice, separate premium rates were not charged for male and female employees but a composite rate was computed at the effective date of the group policy depending on the percentage of female employees covered. If less than 11 per cent of the coverage was on female employees, the premium rate was not increased above the male rate of $.35. If more than 11 per cent and less than 21 per cent of the coverage was on female employees, the basic male rate was increased by 15 per cent. If the percentage of coverage on females was more than 21 per cent and less than 31 per cent, the basic male rate was increased by 25 per cent and so on.

Insurance was also to be offered for the wives and children of employees, and so a separate premium rate had to be calculated for family coverage. A gross monthly cost of $.70 was arrived at for non-

maternity surgery for a dependent wife and a cost of $.60 for obstetrical benefits. For a child, the gross monthly cost was estimated to be the same as for a male employee, namely $.35. It was decided to charge the same premium for each family covered regardless of how many children the family included; and for this purpose it was assumed that, on the average, a dependent family unit would be composed of a wife and two children. Thus, the gross monthly premium rate for dependents became $1.40 per family unit where obstetrical benefits were not to be included and $2.00 per family unit where obstetrics were to be covered.

The initial basic monthly premium rate for employee coverage was subsequently increased from $.35 to $.40 when the benefits were extended to cover out-of-hospital as well as in-hospital surgery and has since remained unchanged. The rates for family coverage were progressively reduced as experience developed and it was seen that they were redundant. The present gross monthly rates for the same company are $1.20 per family unit without obstetrical benefits and $1.55 including obstetrical benefits.

The construction of initial premium scales for the group medical expense insurance plans was similar to the calculation described in connection with surgical benefits. However, the statistical basis was even less satisfactory. It was necessary to fall back to a greater extent on studies made of the utilization of medical care by uninsured individuals. What there were of such studies were usually quite limited in their usefulness. Generally, the number of persons under observation was small the locale of the group being investigated was restricted and the amount of pertinent information secured was meager. Furthermore, since these studies were undertaken mostly from the point of view of the economic theorist or sociologist, they are not very easily adapted to the requirements of the insurance actuary.

A notable exception was a study conducted during the period from 1928 to 1931 on a sample of 9,000 families, chosen geographically and in income groups so as to be representative of the white population of the nation. Each family was interviewed at intervals over a twelve months period in order to secure detailed information regarding the incidence of illness and the extent and kind of medical care sought in connection with it. The statistical data were analyzed and published in the United States Public Health Reports and also were used extensively in the series of monographs published by the Committee on the Costs of Medical Care.

Much caution has to be exercised in using investigations based on uninsured lives for insurance purposes. The methods of collecting and classifying the data must be reviewed, because, for example, an

enumeration process such as employed in the study of 9,000 families tends to underestimate the true figures. The period covered by such a study has also to be considered in the light of the rapidity with which hospital and medical practices are changing.

Of more importance in viewing studies among uninsured persons is the fact that the very presence of insurance benefits materially influences utilization. A ready example is seen in the growth of hospitalization insurance as one of the factors contributing to the overcrowding of hospitals. The use of hospital and medical facilities is, to a large extent, under the control of the individual and his physician. It is true that the degree of control is less for hospital confinement and surgical operations than for home and office calls or diagnostic x-rays. Nevertheless, an increase in utilization rates is to be anticipated under any hospital or medical expense insurance plan; this is a usual consequence of the reduction of the cost which would otherwise be a deterrent to the demand for services and their use. The greater the degree of control on the part of the insured and the doctor, the greater this effect may be expected to be. Moreover, experience demonstrates that each group insurance coverage tends to produce its own characteristic rates of utilization and related statistical factors. For one thing, the claim experience is affected by the practices of the employers and the insurance carrier in administering the insurance contracts.

After a plan of insurance of the type being discussed has been developed and has begun to be sold, the next questions are: how is the experience to be watched and what statistics are to be compiled? Since these plans do not involve the accumulation of funds to offset the future cost of deferred benefits, as in the case of life insurance or annuities, a simple and very effective index of experience may be obtained by a comparison of premiums which become due with the claims that are incurred. Such a comparison brings out very readily the financial effect of the plan on the company's operations and is easy to compute from month to month and to maintain on a current basis. However, such ratios do little to satisfy the need for more detailed statistics for rate making and other similar purposes.

Group insurance policies are written for a period of one year and are renewable for annual periods thereafter. The premium rates at issue are guaranteed for the first policy year but are subject to re-rating upward or downward by the insurance company at the end of that period and at the end of each succeeding policy year. The policies participate in the surplus earnings of the company annually through the declaration of dividends or the allowance of retroactive rate credits, where earned.

Involved in the computation of dividends or retroactive rate credits is the process known as experience-rating, whereby weight is given both to the claim experience of the individual group policy and to the claim experience of the class of policies to which it belongs. It is clear that in order to make possible renewal underwriting and the computation of dividends or retroactive rate credits, a record of premiums due and the number and amount of claims incurred must be kept separately for each group policy for each policy year.

It is natural then that these individual group experience records should be the basis of further statistical analysis within a company. Each group policy can be classified in a number of ways: separately as to the coverage of employees and family units; according to the basic type of coverage it provides; according to the variations in specific benefit provisions, such as whether the maximum period of coverage for hospital expenses is 31 days or 70 days in any one confinement; according to the percentage of female employees covered; according to the industry of the employer, and by geographical location. Thus, a compilation of data regarding claims and premiums by policy years makes possible an extensive statistical analysis. Annual dollar claim costs per unit of benefit can be produced, as well as annual claim frequencies per insured person or family, and the average amounts of benefits paid per claim. These can be obtained separately for male employees, for female employees and for family units. In addition, the variation in these factors by industry, policy duration, calendar year and geographical location may be studied.

This further analysis still does not give all the statistical information necessary for various purposes because it does not involve the classification of individual claims other than by the type of coverage and the group policy under which they were incurred. No information is furnished concerning the variation in claim frequencies, for example, by age, by cause of disability or type of surgical operation, or by duration of disability or period of hospitalization. Only the appropriate classification of individual claims upon their completion and the analysis of their relationship to the numbers exposed to risk can produce such information accurately.

Here arises a question of approach about which there may be some difference of opinion among the insuring companies. The question is whether there should be set up, on a continuing basis for statistical purposes, a system of coding individual claims, involving the use of all or a specific fraction of all claims. The alternative is, of course, to develop the basic claim rates and claim costs on a continuing basis but to

rely on occasional sample studies designed for specific purposes for any additional detailed information which might be desired.

The merits of the question revolve in good part around the cost of the two approaches and can be brought into focus by mentioning that currently one company is paying annually about 75,000 hospital expense claims, 65,000 surgical claims and 45,000 medical expense claims. In the writer's opinion, the additional detailed information does not warrant the cost of securing it on a continuing basis as against the lesser cost of conducting periodic or occasional sample studies. Of course, some sample studies would have to be made anyway because the wide variety of information desired from time to time would not all justify inclusion in any continuous system.

So far, the discussion has been in terms of the operations of a single company. Whenever possible, it is desirable to improve the reliability of statistical results by increasing the body of data available for study. Thus, the combined experience of a number of companies is not only more reliable than that of a single company but it also furnishes a standard by means of which any one company may judge the efficiency of its operations. The medium for the compilation and analysis of group insurance experience on an intercompany basis is a standing committee of the Actuarial Society of America known as the Committee to Prepare Mortality and Morbidity Studies on Group Insurance.

The work of this Committee was interrupted by the war, as were many other statistical efforts of the individual companies. Accordingly, it is only now, after discussions held during the past 18 months, that the Committee has completed plans for the compilation of intercompany group morbidity experience. The contributing companies will be furnishing the Committee with aggregate "claims" and "exposed to risk" for each group policy appropriately classified so as to enable the derivation of unit claim costs. It is expected that the Committee's first report on group morbidity experience will be published in the 1948 *Transactions* of the Actuarial Society. For the time being, group medical expense insurance is not being studied, largely because of the newness of the coverage, the wide variety of plans being written, and the diversity in policy provisions. It is hoped that it will be possible to commence studying group medical expense insurance on an intercompany basis at a later date.

The Committee has also undertaken a sample study based on individual surgical claims. The study is designed to supply information regarding, first, the relative frequency with which different surgical procedures occur and, secondly, the relative values of the fees being

charged by doctors for performing such procedures. There is little known about either of these matters. A detailed frequency distribution of various types of operations should be on hand for the computation of premiums for a given surgical schedule in its entirety, as well as to enable the computation of the cost of a variation in benefit that might be under consideration for a particular surgical operation.

With respect to doctors' actual charges, the companies are interested not so much in their dollar value since it is not contemplated that the surgeon's fee will necessarily be covered in full by the insurance. The companies are concerned instead, with the relationships which doctors' charges for different operations bear to one another. For, if surgical insurance is to be most effective, it is important that the maximum amount of reimbursement provided for each type of operation be reasonably proportionate to the surgeon's fee. Thus, it is not desirable to have a surgical schedule which might on the average insure the full cost of tonsillectomies, for example, while covering only a small fraction of the average surgeon's charge for performing an appendectomy. A scale of the relative values of actual charges will make it possible to determine whether existing surgical schedules are in accord with the pattern of the fees being charged by physicians, and will permit the revision of the schedules where they may not be.

In planning the intercompany study, one of the problems encountered was that of devising a suitable classification code for the various surgical procedures. A code was desired that would be neither too detailed to be practical nor insufficiently detailed for adequate analysis. The information was to be coded from claim papers which do not always give complete medical descriptions. Furthermore, the bulk of the coding work would be done by clerks whose medical knowledge was limited, although the assistance of the insurance company doctors would be available. With the advice of several company doctors, a satisfactory code containing about 400 classifications was prepared.

In order that the study of the relative frequencies of various surgical procedures be free of seasonal bias, it was necessary that the claims included in that portion of the study be chosen proportionately over a twelve months' period. Several of the companies found it inconvenient to select claims in that way and so are participating only in the study of the relative charges made by doctors.

In all, punched cards covering about 100,000 claims have been prepared by seven companies. The following information has been submitted with respect to each surgical claim: (1) the code classification of the first surgical operation performed, (2) the classification of a second

operation which may have been performed at the same time, (3) the total number of procedures performed during the same operation, (4) whether the operation was performed in a hospital or elsewhere, (5) the claimant's sex, including whether an employee or dependent, age and state of residence, (6) the total amount of the doctor's charge, and (7) the amount of the insurance benefit payable. The data from the seven companies have been received by the compiling company and are in the process of analysis. The indications are that the study[1] will produce much information useful not only to insurance companies but also to others.

In concluding, it should be emphasized that insurance against the cost of medical care is relatively new. Much is yet to be learned, especially about plans insuring doctors' charges for non-surgical services; these are very definitely in the formative, experimental stages. The insurance companies are finding out what demand there is for various medical services, what it costs to provide such services and what administrative problems are involved in covering those costs on a prepayment basis. It is hoped that the statistics and experience of the insurance companies will contribute valuable knowledge regarding the important and difficult problem of distributing and adequately paying for medical services in this country.

[1] See "Group Surgical Claims Study, Report of the Committee to Prepare Mortality and Morbidity Studies on Group Insurance," *Transactions* of the Actuarial Society of America, Vol. 49, 1948.

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van Steenwyk, E. A., "The Administration and Underwriting of Hospital and Medical Insurance," *Health Insurance in America;* Address at the Second National Conference on Social Security, January 1945.

PROCEEDINGS

107th ANNUAL MEETING

HOTEL COMMODORE, NEW YORK CITY

PROGRAM

Sunday, December 28, 1947

—9:00 A.M.—

REGISTRATION

—10:00 A.M.—

STATUS AND PROSPECTS IN METHODS OF SOCIAL RESEARCH

(Joint with the American Sociological Society)

Chairman: Raymond V. Bowers, Research and Development Board

Discussion: Stuart C. Dodd, University of Washington
Philip M. Hauser, University of Chicago
Talcott Parsons, Harvard University

THE TEACHING OF STATISTICS

(Joint with the Institute of Mathematical Statistics)

Chairman: William G. Cochran, Institute of Statistics, University of North Carolina

A Review of the Present Situation and Recommendations
A Digest of Recent Reports
James D. Paris, Metropolitan Life Insurance Co.
(Prepared for the American Statistical Association Temporary Committee on Training of Statisticians)

Discussion: Robert W. Burgess, Western Electric Co.
Burton H. Camp, Wesleyan University
Joseph F. Daly, Bureau of the Census
Philip J. Rulon, Harvard Graduate School of Education

PROGRESS IN THE INDUSTRIAL CLASSIFICATION OF ESTABLISHMENTS

Chairman: Emil D. Schell, Bureau of Labor Statistics

Introductory Remarks, V. S. Kolesnikoff, Bureau of the Budget
The Revised Standard Industrial Classification Structure
Frank Cizar, Bureau of the Budget
The Applications of the Standard Structure
Margaret Martin, Bureau of the Budget

The Coordination of Federal and State Social Security Classifications
William Cummins, Bureau of Old Age and Survivors Insurance
Frank Nagel, Bureau of Employment Security

CLASSIFICATION PROBLEMS IN THE 1947 CENSUS OF MANUFACTURES

Discussion: Meredith Givens, New York State Department of Labor
Samuel P. Hayes, Jr., Dun and Bradstreet
Loring Wood, Bureau of the Budget

—1:15 P.M.—

STATUS AND PROSPECTS IN POPULATION PLANS

(Joint with the American Sociological Society)

Chairman: Philip M. Hauser, University of Chicago

Discussion: Frank Notestein, Princeton University
Henry S. Shryock, Jr., Bureau of the Census
Irene Taeuber, Princeton University
P. K. Whelpton, Scripps Foundation for Research in Population Problems

ESTABLISHMENT SAMPLING—A ROUND TABLE

Chairman: Lester S. Kellogg, Bureau of Labor Statistics
Lester Frankel, Dun and Bradstreet
Margaret Gurney, Bureau of the Census
Samuel Weiss, Bureau of Labor Statistics

THE TEACHING OF STATISTICS

(Joint with the Institute of Mathematical Statistics)

Chairman: Francis G. Cornell, University of Illinois

Syllabus for a Proposed Course in Basic Statistics
George W. Snedecor, Iowa State College

Discussion: Elmer B. Mode, Boston University
Samuel A. Stouffer, Harvard University
Helen M. Walker, Teachers College, Columbia University
Albert E. Waugh, University of Connecticut

Film: The Presentation of a film on Modern Quality Control
Simon Collier, Director of Quality Control, Johns Manville Company

—3:30 P.M.—

INTERNATIONAL PROBLEMS IN HEALTH AND VITAL STATISTICS

(Biometrics Section of the American Statistical Association Joint with the Biometric Society)

Chairman: Halbert L. Dunn, National Office of Vital Statistics

Principles Underlying the Sixth Decennial Revision of the International Lists of Diseases and Causes of Death and their Potential Uses
Lowell J. Reed, Johns Hopkins University

Review of Work and Actions of the Expert Committee for the Preparation of the Sixth Decennial Revision of the International Lists of Diseases and Causes of Death of the World Health Organization
W. Thurber Fales, Baltimore City Health Department
Tabulations Needed for International Purposes in the Fields of Health and Vital Statistics
Forrest E. Linder, Statistical Office of the United Nations
The Census Base for Satisfying Health and Vital Statistics Needs
Calvert L. Dedrick, Bureau of the Census

PATTERNS OF WORKING LIFE

Chairman: Gladys Palmer, University of Pennsylvania

Labor Force Life Tables
Seymour L. Wolfbein, Bureau of Labor Statistics
Experience Patterns in Covered Employment
J. Pearlman, Bureau of Old Age and Survivors Insurance
Patterns of Occupational Mobility
Lloyd Reynolds, Yale University

Discussion: Margaret Hagood, Bureau of Agricultural Economics
A. J. Jaffe, Bureau of the Census
Paul Webbink, Social Science Research Council
Emmett H. Welch, Bureau of the Census

MEASUREMENT AND SIGNIFICANCE OF INVENTORIES

Chairman: Ralph J. Watkins, Dun and Bradstreet

Measuring Physical Inventories
Weir M. Brown, Board of Governors of the Federal Reserve System
Significance of Inventories in the Economic Picture
Louis J. Paradiso, Econometric Institute

Monday, December 29, 1947

—9:00 A.M.—

OPERATIONAL RESEARCH

(Joint with the Institute of Mathematical Statistics, with the cooperation of the Operations Evaluation Group of the Navy and Operations Analysis of the Air Force)

Chairman: Edward L. Bowles, Massachusetts Institute of Technology

Operations Research in the Department of the Navy
J. Steinhardt, Director, Operations Evaluation Group
Operational Research in the Department of the Air Force
Leroy A. Brothers, Chief, Operations Analysis

Discussion: Arthur A. Brown, Operations Evaluation Group
Thomas I. Edwards, Operations Analysis
Galen B. Price, University of Kansas
W. J. Youden, Douglas Aircraft Company

MORE ADEQUATE CURRENT REPORTS ON INDUSTRIAL ACTIVITY

Chairman: David Novick, University of Puerto Rico

Speakers: Maxwell Conklin, Bureau of the Census
Edward Hincks, Bureau of the Budget
Geoffrey H. Moore, National Bureau of Economic Research
Nathan Weinberg, United Automobile Workers, Congress of Industrial Organizations

Discussion: Donald Kershner, Crown Central Petroleum
Victor Perlo, Monetary Research Division, U.S. Treasury Department
Lorman Trueblood, Board of Governors, Federal Reserve System

—9:30 A.M.—

SOME ASPECTS OF GRAPHIC PRESENTATION

Chairman: Kenneth W. Haemer, American Telephone and Telegraph Co.

Elements of Presentation
H. C. Barton, Jr., Presentation, Inc.
The Place of Graphics in the Teaching of Statistics
Henry S. Miller, Queens College
Production of Display Presentation
Charles J. Hauk, Jr., Chartmakers, Inc.

—10:00 A.M.—

STATISTICAL PROBLEMS IN HIGHWAY PLANNING AND TRANSPORT RESEARCH

Statistics in Highway Planning and Transport Research
John T. Lynch, Public Roads Administration
Factors Affecting Generation and Distribution of Highway Traffic
Nathan Cherniack, Port of New York Authority
Application of Poisson Distribution Theory to Traffic Control
Donald Schapiro, Yale Bureau of Highway Traffic
Analysis of Motor Vehicle Registration Records
G. P. St. Clair, Division of Financial and Administrative Research, Public Roads Administration

—11:00 A.M.—

THE CURRENT STATUS OF FEDERAL STATISTICS—A ROUND TABLE

Chairman: Stuart A. Rice, Division of Statistical Standards
Ewan Clague, Bureau of Labor Statistics
Howard C. Grieves, Bureau of the Census
Orris V. Wells, Bureau of Agricultural Economics

—12:00 M.—

COMMITTEE ON CENSUS ENUMERATION AREAS—*Luncheon*

Chairman: Howard Whipple Green, Cleveland Health Council

Plans for the 1950 Population Census Related to Census Tracts and Block Data
Leon E. Truesdell, Bureau of the Census

Tracted Cities in Canada
O. A. Lemieux, Dominion Bureau of Statistics, Ottawa, Canada
Use of Data by Blocks in St. Louis
Edward B. Olds, Metropolitan St. Louis Census Committee
General Discussion: Series of Four Minute Reports from Census Tract Key Persons

—1:15 P.M.—

SOCIAL PSYCHOLOGY

(Joint with the American Sociological Society)

Chairman: Herbert Goldhamer, University of Chicago

Discussion: Joseph K. Folsom, Vassar College
Abraham Kardiner, New York City
Richard T. LaPierre, Stanford University
C. Wright Mills, Bureau of Applied Social Research

MORE ACCURATE INDUSTRIAL STATISTICS

Chairman: David Novick, University of Puerto Rico

Melvin Anshen, Indiana University
Solomon Barkin, Textile Workers Union
Ewan Clague, Bureau of Labor Statistics
F. W. Heintzelman, American Institute of Accountants

Discussion: G. A. Renard, National Association of Purchasing Agents
Boris Shishkin, American Federation of Labor
George A. Steiner, University of Illinois

—3:15 P.M.—

BIOMETRICS SECTION—CONTRIBUTED PAPERS

Chairman: J. H. Watkins, Yale University

Triple Rectangular Lattices
Boyd Harshbarger, Virginia Polytechnic Institute
Estimation of the Mean and its Error from Grouped Poisson Distributions
C. I. Bliss and M. C. Jackman, Connecticut Agricultural Experiment Station
On the Use of Parallel or Nonparallel Systems of Transformed Curves in Bio-assay
William R. Thompson, New York State Department of Health
Observations on Cost Utility Rating in Relation to Correlation
Joseph Berkson, Mayo Clinic
"Use of Statistical Methods in Human Genetics." *A Test for Monomeric Inheritance of a Normal Trait in Man*
Marianne E. Bernstein, Syracuse University

—3:30 P.M.—

OPERATIONAL RESEARCH AND OTHER MILITARY APPLICATIONS OF STATISTICS

(Joint with the Institute of Mathematical Statistics, with the cooperation of the Operations Evaluation Group of the Navy and Operations Analysis of the Air Force)

Chairman: Merrill M. Flood, General Staff, United States Army

Operations Analysis in the Southwest Pacific Air War
Roger I. Wilkinson, Bell Telephone Laboratories
Operations Analysis of Air-Sea Rescue
E. S. Lamar, Operations Evaluation Group
Factorial Chi-Square in Test Shooting
A. E. Brandt, Naval Ordnance Laboratory
Mathematical Techniques of Program Planning
George Dantzig, Consultant to the Air Comptroller, Headquarters, United States Air Force

MEASURES OF CONSTRUCTION ACTIVITY

Chairman: Paul Krueger, Bureau of the Budget

A User's View
Leo Grebler, Private Consultant
The Government's Estimates of Residential Building
H. E. Riley, Bureau of Labor Statistics

Discussion: Howard Whipple Green, Cleveland Health Council
W. A. Berridge, Metropolitan Life Insurance Co.
Samuel Dennis, Department of Commerce
W. G. Shaw, Department of Commerce
Clyde Shute, F. W. Dodge, Inc.

INSTITUTE OF MATHEMATICAL STATISTICS—BUSINESS MEETING

—5:30 P.M.—

AMERICAN STATISTICAL ASSOCIATION—ANNUAL BUSIENSS MEETING

—8:00 P.M.—

PRESIDENTIAL ADDRESSES

(Joint with the American Sociological Society)

Willard L. Thorp, President, American Statistical Association
Louis Wirth, President, Americal Sociological Society

Tuesday, December 30, 1947

—9:00 A.M.—

HOUSING

(Joint with the American Sociological Society)

Chairman: Howard Brunsman, Bureau of the Census

An Appraisal Method for Measuring the Quality of Housing
Allan A. Twichell, American Public Health Association Committee on the Hygiene of Housing
Problems in Development and Application of Standards of Housing in Making Adequate Budgets
Dorothy S. Brady, Bureau of Labor Statistics

Discussion: John P. Dean, Research Fellow, Social Science Research Council
Nicholas J. Demerath, University of North Carolina

—10:00 A.M.—

DESCRIPTIVE STATISTIC IN THE SERVICE OF LIFE INSURANCE

Chairman: Joseph B. MacLean formerly Vice President and Actuary of the Mutual Life Insurance Company of New York and past President of the Actuarial Society of America

Life Insurance Investments and the Capital Markets
Fergus J. McDiarmid, Lincoln National Life Insurance Company
Mortality Statistics for Life Insurance Underwriting
Edward A. Lew, Metropolitan Life Insurance Company
Statistical Problems in New Fields of Insurance
Morton D. Miller, Equitable Life Assurance Society

—9:30 A.M.—

APPLICATION OF THE THEORY OF EXTREME VALUES

(Joint with the Institute of Mathematical Statistics)

Applications to Fracture Problems
Benjamin Epstein, Carnegie Institute of Technology
Applications to the Prediction of Flood Flows
Emil Gumbel, Brooklyn College
The Mathematical Theory of Extreme Values
Richard von Mises, Harvard University
Applications to Meteorology
Horace Norton, Weather Bureau

—11:00 A.M.—

PROGRESS REPORT AND DISCUSSION OF PRODUCTIVITY IN INDUSTRY

Ernest Dale, Columbia University, American Management Association
Katherine P. Ellickson, Congress of Industrial Organizations
W. D. Evans, Bureau of Labor Statistics
Martin Gainsbrugh, The Conference Board
Thomas Mills, Bureau of the Budget
Lazare Teper, International Ladies Garment Workers Union

—1:00 P.M.—

OUTLOOK FOR 1948

Chairman: Willard L. Thorp, President, American Statistical Association

Discussion: Martin Gainsbrugh, The Conference Board
James Hughes, Auchincloss, Parker and Redpath
Ragnar D. Naess, Naess and Commings
Imrie de Vegh, Consulting Economist

BIOMETRICS SECTION—BUSINESS MEETING

STATISTICAL TECHNIQUES IN LIFE INSURANCE

(Joint with the Institute of Mathematical Statistics)

Chairman: Robert J. Myers, Social Security Administration

Problems with Sampling Procedures for Reserve Valuations
George C. Campbell, Metropolitan Life Insurance Co.
Sampling Errors in Life Insurance Mortality and Other Statistics
Donald Cody, Equitable Life Assurance Society
Recent Developments in Graduation and Interpolation
T. N. E. Greville, National Office of Vital Statistics

—3:30 P.M.—

CENSUS AND VITAL STATISTICS

(Joint with the American Sociological Society)

Chairman: Edmund de S. Brunner, Columbia University

Sociologists' Suggestions Relating to the 1950 Census in the U.S.
P. K. Whelpton, Scripps Foundation for Research in Population Problems
Comments on Feasibility of the Sociologists' Suggestions
A. Ross Eckler, Bureau of the Census
Problems in Revision of the Birth and Death Certificates
Felix Moore, National Office of Vital Statistics
Discussion: T. Lynn Smith, Vanderbilt University
Dorothy Swaine Thomas, Giannini Foundation

THE USE OF STATISTICS BY BUSINESS MANAGEMENT—A ROUND TABLE

Chairman: Isador Lubin, United Nations Labor and Employment Commission
Robert W. Burgess, Western Electric Co.
W. H. Garbade, Shell Oil Co.
Richard O. Lang, S. C. Johnson & Son, Inc.
Charles Roos, Econometric Institute
Clarence Smith, International Business Machines

AMERICAN STATISTICAL ASSOCIATION—CONTRIBUTED PAPERS

Factors in the Discontinuance of a Statistical Series
Harry Alpert, Bureau of the Budget
Testing for Grade and School Differences Among the Scores of Home Economics Students
William D. Baten, Brooklyn College
On Measuring Languages for International Use: On Pan Sampling
Stuart C. Dodd, University of Washington Public Opinion Laboratory
Sampling Errors and Components of Interviewer Costs in Survey Design
Roe Goodman and Charles Cannell, Survey Research Center
The Sugar Consumption Quota: A Problem in Administration of Legislative Controls
Julius Guttenplan, Sucrest Corp.
Income, Savings, and Mortgage Investment
Mortimer Kaplan, Federal Housing Administration

Objective Selection of a Respondent within the Household
Leslie Kish, Survey Research Center, University of Michigan
Trends in Statistical Activities of Trade Associations
Sidney Lieberman, Chart Facts
Maximum Likelihood Estimates of Absolute Dispersion for Samples Drawn from Type III and Type V Populations
Nilan Norris, Hunter College

INSTITUTE OF MATHEMATICAL STATISTICS—CONTRIBUTED PAPERS

THE BIOMETRIC SOCIETY, EASTERN NORTH AMERICAN REGION—BUSINESS MEETING

List of Committee Chairmen and Representatives for 1947

Committee on Fellows: Joseph S. Davis
Committee on Nominations: Woodlief Thomas
Committee on Graphics: Kenneth W. Haemer
1947 Annual Meeting Program Committee: Samuel S. Wilks
International Statistical Conferences ASA Program Committee: Willard L. Thorp, Philip M. Hauser, *Acting Chairman*
Committee on Census Enumeration Areas: Howard W. Green
Joint Committee on Occupational Classification: Gladys L. Palmer
Census Advisory Committee: W. F. Ogburn
Advisory Committee to BLS on Employment and Statistics Program: S. S. Wilks
Advisory Committee to the National Housing Agency on Housing Statistics: Howard G. Brunsman
Biometrics Section Committee: D. B. DeLury
Temporary Committee on the Training of Statisticians: F. G. Cornell
Representatives on the Committee for the Preparation of a Source Book of Economic Statistics: Walter Mitchell, Jr., J. Frederic Dewhurst
Representative on the Board of Directors of the National Bureau of Economic Research: Frederick C. Mills
Members of the Social Science Research Council: Joseph S. Davis, Harold Hotelling, Willard L. Thorp
Representative on the Council of the American Association for the Advancement of Science: Frederick F. Stephan
Representatives on the Joint Committee for the Development of Statistical Applications in Engineering and Manufacturing: Churchill Eisenhart, Walter A. Shewhart
Representative on the Sectional Committee on Standards for Graphic Presentation of the American Standards Association Sponsored by the American Society of Mechanical Engineers: Kenneth W. Haemer
Representative on the Advisory Board of the American Year Book: Frank W. Notestein
Representatives on the Intersociety Committee on Science Foundation Legislation: Lester S. Kellogg, Frederick F. Stephan
Member of the American Documentation Institute: Lester S. Kellogg

MINUTES OF THE ANNUAL BUSINESS MEETING

The American Statistical Association convened for its 107th Annual Business Meeting at the Hotel Commodore, New York City, Main Ballroom, on Monday, December 29, 1947, President Thorp presiding.

The minutes of the 106th Annual Business Meeting, held in Atlantic City on January 25, 1947 were approved.

Aryness Joy Wickens, acting for Joseph Davis, Chairman of the Committee on Fellows, read the following citations of the Fellows elected for 1947:

The Committee on Fellows has unanimously elected six members as Fellows of the American Statistical Association:

John Hamilton Curtiss, Assistant to the Director of the National Bureau of Standards; contributor to statistical theory and to pure mathematics; teacher, administrator, and patriotic innovator in the application of statistical methods to problems of the Navy at war.

Milton Gilbert, Chief of the National Income Division of the Bureau of Foreign and Domestic Commerce of the U. S. Department of Commerce, and formerly Editor of the *Survey of Current Business*. He has been responsible for much of the improvement of statistics of gross national product and has contributed notably to the analysis of the national income.

Morris H. Hansen and William Hurwitz. U. S. Bureau of the Census, a distinguished statistical team which, although among the younger group of outstanding members of this society, has made important contributions to the use of sampling techniques in the recurrent report of the Census Bureau on population and unemployment. They are joint developers of large-scale techniques whereby gamblers' tools can serve solid citizens through efficient gathering of pertinent information.

Jacob Marschak, Research Director of the Cowles Commission and Professor of Economics at the University of Chicago; born in Russia, with academic degrees from Heidelberg and Oxford, he has made valuable personal contributions and exercised constructive leadership in statistics, economics, and econometrics in England and in the United States.

Herbert Marshall, Dominion Statistician of Canada, Chairman of United Nations Statistical Commission, noted for his responsible leadership in and important contributions to Canadian statistics, especially those on the Canadian international balance of payments and on Canadian-American direct investments; and for his services in the use of statistics in the control of his country's economy during a great war.

Lester S. Kellogg, Secretary of the Association, reported briefly on the current status of finances and membership of the Association.

The following resolution, submitted by A. Ford Hinrichs, was read:

RESOLVED that:

1. The President of the American Statistical Association shall arrange in consultation with the President of the American Economic Association that the annual meetings in 1949 and later years shall be held at a common time and place.
2. In order to contribute to a unified attack on common problems, other

societies of the social sciences should be invited to join in the arrangements for a common meeting place.

3. The President of the American Statistical Association shall appoint a Committee not later than March, 1948, to arrange a program on the PROBLEMS OF THE STATISTICAL PROFESSION and of a unified attack on problems in the field of the social sciences, to constitute the core of the sessions of the annual meetings in 1949. The Committee shall have discretion as to the contents and arrangement of this program, but it is desirable that it should include a discussion of:

a) professional standards and the professional requirements of various fields of work and types of occupations in which statisticians are commonly engaged.
b) training requirements and methods, with reference not only to professional statisticians but also with reference to the elementary levels for citizenship and active participation in a democracy.
c) kind of professional organization needed to encourage the development within the total profession of groups of specialized professional interests.
d) methods of presenting the findings of the profession to its members and of presenting these findings and representing the profession to the public.
e) proposals for the development of a better coordinated attack by the profession on unsolved problems of statistical theory, including
   i) the development of a moving 5-year program to establish the goals of the Association itself,
   ii) a continuing analysis in each field of theory of the gaps in the data or in professional knowledge,
   iii) arrangements to encourage research and analysis particularly at these critical points,
   iv) arrangements at the annual meetings to present a comprehensive brief view of all new advances to the profession, with special reference to advances in statistical theory, in the measurement of relationships between variables, in data and in techniques of analysis.

While this program shall constitute the core of the annual meeting in 1949, it is not intended to exclude other types of discussion.

4. The Committee so appointed shall invite the participation of the Social Science Research Council in its deliberations and program arrangements and shall consult with committees that may be appointed for similar purposes by other associations of the social sciences. As far as possible, joint programs shall be arranged with such other associations to deal with common or similar problems.

This resolution was referred to the Board of Directors for its consideration. (A similar resolution was presented at the annual meeting of the American Economic Association and the American Sociological Society.)

Willard L. Thorp, President reporting for the Committee on Structure, stated that the draft of the new Constitution was in process of revision and would be submitted to the members within a short time. He informed the membership that through an oversight of the Board of Directors a proposed amendment to the Constitution, approved by the membership in 1940, was never resubmitted to the membership because of the failure to hold annual meetings during the

recent world war. This report was noted as a matter of information and no additional vote was taken.

In the absence of Francis G. Cornell, Chairman of the Temporary Committee on the Training of Statisticians, Helen M. Walker, a member of the Committee, reported that the Committee had organized several sessions at the Annual Meeting which had been very well attended. Over 70 people present at these sessions on training signed a petition for the formation of a Section on the Training of Statisticians, and Professor Walker announced that an organization meeting to form the Section would be held the following day.

Samuel S. Wilks reported that a plan had been worked out for the publication of a new periodical to be sponsored by the Association, to be called "Statistical Reviews." The publication will begin its appearance in 1949 and will carry book reviews, abstracts, and the Statistical Methodology Index which at present appears in the *Journal*. He reported that plans had been completed and were ready to submit to a foundation with a request for subsidy.

Dr. Wilks also reported on the activities of the Committee to advise the Bureau of Labor Statistics on its employment statistics program. This Committee prepared a detailed report which included a number of useful recommendations and was submitted to the Commissioner of Labor Statistics in August of 1947. Aryness Joy Wickens, of the Bureau of Labor Statistics, expressed the thanks of the Bureau for the service rendered by the Committee.

Willard L. Thorp presented the following slate of officers prepared by the Nominating Committee:

George W. Snedecor, President;

Frederick F. Stephan and Willard L. Thorp, Directors for the years 1948, 1949 and 1950;

A. Ford Hinrichs, Rensis Likert, Abraham Wald, and Harry R. Wellman, Vice-Presidents;

Lester S. Kellogg, Secretary-Treasurer.

The vote in favor of the slate was unanimous and the Secretary was instructed to cast one ballot for the entire list.

Willard L. Thorp, outgoing President, turned the Chairmanship of the meeting over to the newly elected President, George W. Snedecor. Professor Snedecor expressed his pleasure at the honor conferred on him and his appreciation of the diligent and unremitting efforts of Lester S. Kellogg as Secretary of the Association.

The following resolution was presented. It is RESOLVED that the Officers and the members of the Association express their appreciation to the New York Chapter for its help in making such excellent arrangements for the Annual Meeting. The thanks of the membership are particularly due to Waite S. Brush, District Representative of the New York Chapter and Chairman of the local Committee on Arrangements, for his effective assistance.

It was also RESOLVED to commend Waite S. Brush for the remarkable expansion of activities in the New York area during the past year.

Solomon Barkin presented a resolution stating that "The American Statistical Association should investigate its responsibilities in the field of industrial statistics." This resolution was referred to the Board of Directors for action.

Waite S. Brush reported that there had been a tremendous increase in activities in the New York area. He reported that the Social Statistics Section and the Sta-

tistical Techniques Section of the Chapter had been meeting regularly, that a number of full Chapter Meetings had been organized and well-attended during the course of the past year. The Chapter has been occupied with the statistical problems of the New York area and has been working with municipal and state committees to determine statistical requirements in New York and in helping to plan means of making additional statistical data available. Mr. Brush appealed for help from the national Association in obtaining the services of a paid secretary and asked the attention of the President and the Board of Directors for his request.

The meeting was adjourned.

LESTER S. KELLOGG, *Secretary*

Report of the Board of Directors

The year 1947 witnessed a continuing growth of the American Statistical Association and an expansion and ramification of its activities. The dues-paying membership increased from approximately 3,800 to about 4,800. Three new chapters were organized, in Honolulu, Ithaca and Oklahoma City. Negotiations were begun for the chartering of additional chapters, two of which were formally organized during the first quarter of 1948.

During September 1947, many of the world's leading statisticians met in Washington for the sessions of the International Statistical Conferences. Five hundred scientific workers in statistics and allied fields attended and all major countries, with the exception of the U.S.S.R., sent delegates. The groundwork was laid for the resumption of that fruitful international cooperation among scientists which characterized the pre-World War I and inter-war periods.

The Association was one of the sponsoring organizations of this gathering. In addition to giving a reception at which the foreign delegates were able to renew social and intellectual contacts which had been broken during the War, the ASA organized three of the sessions of the Conferences. A special issue of the *Journal* was prepared for the international meetings and distributed gratis to the foreign delegates. A non-commercial graphics exhibit was organized to familiarize non-United States delegates with the presentation methods developed in this country over the past seven years. Through the cooperation of publishing firms and statistical machine manufacturers, a commercial display was provided which gave comprehensive coverage of textbooks, publications on methodology and specialized fields and mechanical aids to statistical work, ranging from simple computing machines to highly advanced and specialized types. The exhibit produced a net income of $3,286.20, which was made available to the Joint Arrangements Committee to help in defraying expense incident to conducting the Conferences.

Recognizing that physical destruction of foreign libraries, dollar exchange shortages and other factors have conspired to deprive foreign statisticians and statistical institutions of the tools necessary to their work, the Association made available complete sets of the ASA *Journal* for the past 15 years gratis to approximately 25 war-devastated libraries. Current books on statistical developments and methodology were also made available to needy foreign statisticians, scholars and schools.

The International Statistical Conferences in Washington resulted in the organization of several new specialized groups of statisticians and persons working in closely related fields. International groups were launched to study national in-

come and wealth, public opinion sampling, biometrics and demography. In all these new ventures, members of the Association have played an active role.

The papers delivered at the Conferences are to be published in five volumes. The Association is editing the contributions made to the three sessions which it organized. Noteworthy papers of special interest to the membership will be published separately in forthcoming issues of the *Journal.*

In August 1947, the first issue of *The American Statistician* appeared.This bi-monthly periodical, which supplants the organizational *Bulletin* of the Association, was planned as a professional news magazine. It carries brief reports of important statistical news, discussions of professional problems, and articles on the activities of statisticians. The magazine is designed to reach not only the members of the Association but that large group of students and users of statistics who are interested in the activities of the profession. Its circulation at the close of 1947 was around 5,400.

The 107th Meeting of the Association was held in New York City at the Hotel Commodore in December 1947 in conjunction with the American Sociological Society, the Biometric Society and the Institute of Mathematical Statistics. Despite the New York blizzard and the meetings of the American Economic Association and of Section K of the AAAS simultaneously in Chicago, the New York meetings drew an attendance of over 1,000 persons. A feature of the New York meeting was the comprehensive display of statistical presentation methods prepared for the Association by its Committee on Graphics under the chairmanship of Kenneth W. Haemer.

Further discussion of the new Constitution of the Association occurred at the December meeting in New York. The document discussed at that meeting was sent back for redrafting. This redraft was accepted, by mail ballot of the membership, in early 1948.

Plans for a joint Annual Meeting between the American Economic Association and the ASA in Cleveland in the last week of December 1948 have been endorsed by both organizations. Tentative arrangements for a joint annual meeting on the West Coast in 1949 or 1950 are currently under consideration.

The central office of the Association in Washington acquired additional space and staff in 1947 to meet the requirements of continuing ASA growth. At the close of 1947, Lester S. Kellogg, secretary-treasurer of the Association during the period of rapid expansion of the war and post-war years, accepted an executive position with Deere and Company in Moline, Illinois. In view of his absence from Washington, Sylvia C. Weyl was named Acting Secretary-Treasurer on an ad interim basis and a committee was appointed to study the question of finding a successor to Dr. Kellogg.

The development of the statistical field has required increasing activity and responsibility on the part of the Association. There is a demand for more publications, more meetings with other professional groups, more professional representation, an employment service and more committee activity. These activities require more resources and an expanded secretarial staff. Costs of printing and other services have about doubled since the end of the war. The effects of inflation have been partly offset by the growth in membership and subscriptions and the contributed secretarial and editorial services. The old membership rate of $5.00 a year was found to be inadequate to cover basic expenses much less to

permit expanded activity. In view of this situation the membership was asked to increase the rate to $8.00—which was voted in February 1948.

The Biometrics Section of the ASA participated actively in the International Conferences and in the organization of The Biometric Society, the new international organization of biometricians. The publication *Biometrics* completed its first year as a quarterly and was requested to print the Proceedings of the First International Biometric Conference.

The Temporary Committee on the Training of Statisticians, under the chairmanship of Francis G. Cornell, made a report to the Board. As a result of this report, a Section on the Training of Statisticians was organized within the ASA and is charged with the formulation of standards for teaching statistics and with the preparation of special sessions at the Annual Meeting in 1948.

The Association has been actively involved in preparations for the 17th Decennial Census of the United States. The Committee to Advise the Bureau of the Census, which has the responsibility of reviewing all requests from the general public for the inclusion of additional information in census data, has been working as a clearing house between statisticians and government to determine the content of census questionnaires.

The Advisory Committee to the Bureau of Labor Statistics on the Employment Statistics Program submitted a comprehensive and final report in 1947 which was summarized in the April 1948 issue of *The American Statistician*.

The year has been one in which the Association has grown, its responsibilities have increased and the services it renders to the statistical profession have expanded. The influence of ASA publications has been notably enhanced. Thus, the circulation of the *Journal* gained by approximately 20 per cent to reach 5,895. *The American Statistician* is reaching a broad group of less technically and mathematically minded readers, familiarizing them with general developments in the statistical field. In recognition of the probability that the Association must in the coming years provide still broader services, a decision has been taken to launch a new publication, under the editorship of Oscar K. Buros, probably in 1949. This new organ will cover the field of book reviews, will provide an index of statistical methodology, and will make available a comprehensive digest of all significant articles, both domestic and foreign, which deal with statistical problems. The increasing reliance of science, industry, business and government on statistical method, coupled with the continuing development of the science of statistics itself, confronts your Board with the problems incident to endeavoring to meet the unsatisfied needs of a corps of workers in an expanding science.

Lester S. Kellogg
Isador Lubin
Lowell J. Reed
Walter A. Shewhart
Samuel A. Stouffer
Willard L. Thorp
Helen M. Walker
Samuel S. Wilks

The Secretary's Report on Membership

During 1947 membership in the American Statistical Association increased by 1,028, bringing the total on December 31, 1947 to 4,871. Three new corporate members joined the Association. The continued growth during the past year indicates the growing interest in the Association's activities and a large potential membership.

The 1947 membership is composed of the following groups:

Honorary members	15
Fellows	142
Regular members	4,714
Total Membership	4,871
Corporate Members	6

There were 82 resignations in the course of the year, and 189 members were removed from the rolls for failure to pay dues.

The members of the Biometrics Section, at the end of December, 1947, numbered 1,060, of which 262 were associate members and 798 were also members of the ASA.

The deaths of the following members were recorded during the year: Irving Fisher, *Fellow;* Charles A. Bauer, L. A. Beers, H. M. Biggert, Raymond Carpenter, Charles Cresswell, Kenneth Duncan, Albert M. Freeman, Thomas B. Graham, Otto Jeidels, James E. Kelley, Sara A. Kerr, H. G. MacCall, Joseph P. McElroy, Hans Nybolle, Henry A. Ruger, James G. Smith, Philip Sweet, Henry A. Van Pelt, *Regular Members.*

LESTER S. KELLOGG, *Secretary*

Report of the Treasurer

The 1947 budget as approved by the Board of Directors planned for an income of $32,535.00 and expenditure of $35,192.00. At the end of the year actual income was $37,014.87, and our expenditures came to $42,134.72.

The increase in income has come largely from new memberships, increases in subscriptions and additional advertising in the *Journal,* and was substantial enough to permit the publication of a greatly enlarged December issue of the *Journal.* Income from *Biometrics* has increased, and it is expected that its income for the next few years will continue to expand as its service becomes more widely recognized.

The national office is planning a new drive for memberships and subscriptions this year, and the budget will consequently be substantially increased.

LESTER S. KELLOGG, *Treasurer*

Report of the Auditors

To the Board of Directors of
American Statistical Association

We have examined the attached financial statements of American Statistical Association relating to the year ended December 31, 1947. Our examination was made in accordance with generally accepted auditing standards, and included such tests of the accounting records and other supporting evidence and such other procedures as we considered necessary in the circumstances.

The recorded cash receipts for the year were traced to the deposits shown on the bank statements and the amounts for dues and subscriptions were tested with the membership and subscription records. The paid checks were inspected and related vouchers tested in support of cash disbursements for the year. The bank balances were reconciled with amounts reported direct to us by the depositaries and the cash on hand and the securities owned at December 31, 1947 were verified by inspection. We did not check the membership and subscription records in detail or make any independent verification of the inventory of old Journals, the office records of which are based, in part, on data assembled in prior years, no recent physical inventory having been taken.

The life membership reserve is computed on the basis of the combined annuity table of mortality with assumed interest at 4% per annum and an assumed annuity of $5.00 per life member, in accordance with a resolution of the Board of Directors on March 31, 1936. The amount treated as income in each year represents the excess of the reserve at the beginning of the year plus interest for the year and new life membership receipts over the required reserve at the end of the year.

In our opinion, the accompanying statements present fairly the position of American Statistical Association at December 31, 1947 and the results of its operations for the year, in conformity with generally accepted accounting principles applied on a basis consistent in all material respects with that of the preceding year.

PRICE, WATERHOUSE & CO.

American Statistical Association

Balance Sheet

Assets	*December 31, 1947*	*1946*
Cash in bank and on hand	$ 1,853.91	$12,475.91
Accounts receivable	1,356.22	774.19
Investments:		
United States Savings Bonds, Series D, at redemption value	6,138.00	5,910.00
Stocks, at cost (at market quotations $2,150 and $6,358, respectively)	5,793.50	5,793.50
Inventory of old Journals, at approximate cost	2,024.63	2,158.04
Furniture and equipment, at cost less depreciation	2,482.76	847.11
	$19,649.02	$27,958.75
Liabilities		
Accounts payable	$ 6,515.76	$ 3,537.45
Deferred income (collections applicable to subsequent year):		
Dues	$ 1,054.50	$ 7,260.67
Subscriptions	2,704.76	3,158.48
Other	115.35	
	$ 3,874.61	$10,419.15
Net worth:		
Life membership reserve	$ 3,750.20	$ 3,377.35
Centenary Sustaining Fund		6,376.01
Surplus, per statement	5,508.45	4,248.79
	$ 9,258.65	$14,002.15
	$19,649.02	$ 27,958.75

American Statistical Association

Surplus Statement

	Year ending December 31, 1947	*1946*
Balance at beginning of year	$ 4,248.79	$ 4,594.20
Add—Transfer of balance in Centenary Sustaining Fund	6,379.51	
	$10,628.30	$ 4,594.20
Deduct—Excess of expenses over income for the year, per income statement	5,119.85	345.41
Balance at end of year	$ 5,508.45	$ 4,248.79

American Statistical Association
Income Statement

	Year ending December 31, *1947*	*1946*
Income:		
Dues—current year	$21,298.58	$17,452.50
Dues—prior years	215.00	92.00
Life memberships	397.33	352.47
Subscriptions	6,206.95	5,341.30
Advertising	2,010.08	1,700.88
Reprints	509.00	530.93
Journal sales	2,827.80	793.80
Biometrics Section income	2,466.86	2,121.97
Miscellaneous	684.70	394.71
Dividends and interest (after deducting $135.09 in 1947 apportioned to life membership reserve)	398.57	432.08
	$37,014.87	$29,212.64
Expenses:		
Journal—printing, mailing and reprints	$10,105.81	$ 7,908.35
Bulletin	4,259.88	991.01
Salaries and wages	17,037.26	11,876.44
Rent	1,365.00	1,020.00
Office supplies, printing and mimeographing	1,764.42	1,277.44
Postage	1,511.11	1,060.07
Telephone and telegraph	512.03	369.30
Travel expense—officers	603.91	287.29
Biometrics Section expenses	2,366.46	2,185.81
Membership Directory expense		1,575.76
Storage of old Journals	72.00	72.00
Cost of old Journals sold	382.27	58.69
Depreciation of furniture and equipment	324.99	149.49
Annual meeting expense (net)	647.89	238.84
Loss on disposal of equipment	151.09	
Miscellaneous	1,030.60	487.56
	$42,134.72	$29,558.05
Balance, loss, carried to surplus	$(5,119.85)	$ (345.41)

BOOK REVIEWS

Edited by

OSCAR KRISEN BUROS

Rutgers University

Statistical Methods in Research and Production: With Special Reference to the Chemical Industry. Edited by *Owen L. Davies.* Written by *Lewis R. Connor, Wilfred R. Cousins, Owen L. Davies, Francis R. Himsworth, Harold Kenney, Wilfred L. Stevens* and *Charles C. Tanner.* (Chemists, Engineers, and Statisticians employed by Imperial Chemical Industries Ltd., Manchester, England.) Published for the Imperial Chemical Industries Limited. Edinburgh 1, Scotland: Oliver and Boyd Ltd. (Tweeddale Court), 1947. Pp. xii, 292. 28*s.*

REVIEW BY D. B. DELURY
epartment of Mathematical Statistics
AND
A. E. R. WESTMAN
Director, Department of Chemistry
Ontario Research Foundation, Toronto 5, Canada

THE object of the authors is to put into the hands of the industrial research worker a handbook prepared especially for him in terms with which he is likely to be familiar. To this end, the main body of the discussion is given in verbal and arithmetical terms. Whatever algebraic developments are needed appear in appendices at the ends of chapters. This is not to imply that the book is chiefly a guide to effective computational methods. Emphasis is placed on the experimental situation and much careful and thoughtful explanation accompanies each example.

The range of topics covered is indicated by the chapter headings: Frequency Distributions, Averages and Measures of Dispersion, Tests of Significance, Analysis of Variance, Regression and Correlation, Frequency Data and Contingency Tables, Sampling, Control Charts, Prediction and Specification.

Each chapter starts with a general description of the questions with which it deals, followed by examples based on data (usually drawn from the chemical industry) which are intended to illustrate the type of situation with which the chapter deals. The discussion of the examples is not obscured by arithmetical details; systematic methods of computation are dealt with in chapter appendices.

This plan is, no doubt, effective, but starting as it does with the data rather than with the *gathering* of the data it tends to cover up the importance of statistical considerations in the planning of the experiment. Perhaps each chapter should include an example in which statistical techniques can extract no worth-while information from the data!

Those who are interested chiefly in practical applications will find much useful information gathered together in this handbook. On the whole it is

clearly presented and illustrated by good examples. The reader who has a knowledge of theoretical statistics will doubtless find many points on which he does not agree with the authors and others may find some of the discussions confusing. Certainly the quality of the book is uneven with respect to both clarity of exposition and accuracy of statement. While many of the discussions and explanations are excellent, others are obscure, sometimes inaccurate and occasionally meaningless. For example, it is not likely that anyone will be helped to a clearer understanding of statistical estimation by the statement that "Statistical estimation consists, in general, of postulating that a given set of data or *sample* was drawn from a Universe of a certain form. The constants or *parameters* required to define the Universe may be estimated from the data or may be postulated" (p. 14).

Owing, perhaps, to reluctance of the authors to expand the book to greater length, some topics are treated too briefly. Only one model for the analysis of variance is explained, that in which differences among cell averages are regarded as arising entirely from random sources. The case of "fixed effects" is not discussed, although these methods are used to exhibit, for example, differences among three "gaugers," who could hardly be considered a randomly chosen sample from a population of gaugers, in a test of the compressive strength of Portland cement (Example 5.5), and the difference between two operators who carried out simultaneous determinations of the percentage of ammonia in a plant gas (Appendix 4B).

The chapter on regression suffers considerably from the failure to discuss the underlying statistical model. The conditional nature of the regression problem never emerges. Instead, there is created a hierarchy of regression problems, the two extremes of which are, apparently, (*i*) the case in which the quantities observed are inherently variable, in a statistical sense; (*ii*) the case in which the quantities observed are not statistical variables; the only random variation in this case comes from errors of measurement. This is described as the case in which the variables are related by an exact mathematical formula.

The choice of examples to typify these two extremes is unfortunate. Case (*i*) is discussed in terms of the relationship between hardness and abrasion loss in rubber. The observations on these two variables are made on samples of rubber in which the amounts and types of ingredients are deliberately changed from one sample to another, a situation which would seem to require the use of multiple regression methods. Case (*ii*) is illustrated by the dependence on concentration of the response to a penicillin solution. It may be granted that the concentrations are known without appreciable error, but the variations in response represent genuine biological variation and it is difficult to envisage an "exact" relationship between response and concentration. In any case, the differences between these two cases largely disappear when the true nature of regression is recognized.

The distinction between these types is maintained throughout, culminating in two different formulae for the standard error of a prediction based on

a regression equation (Formulae 6.3 and 6.31). It is not clear what is being predicted, a mean value or the value of a future observation, but whichever is intended, the same formula (6.3 *or* 6.31) should apply in both case (*i*) and case (*ii*).

More serious is the failure to bring together regression methods and the analysis of variance, so as to present the elements of the analysis of covariance, a technique second in importance only to the analysis of variance in all fields of experimentation.

Inevitably a few slips must creep into every book. The following may be noted.

(1) The F test is introduced in a comparison of variance estimates from two independent samples, a situation in which a two-tail test usually is indicated. However, the test employed is one-tailed (Example 4.51).

(2) Throughout the discussion of the errors of prediction which result from the errors of the regression coefficients (p. 130), a should be replaced by y.

Readers interested in control chart techniques will find a clearly written chapter on the subject based on British Standard practice.[1] However, only the charts for average and range are discussed in detail and the unfortunate impression is given by some parts of the discussion and by the numerical example that a subgroup of four or five is necessarily "rational." In the chapters concerned with other techniques, not much attention is paid to the quality control approach to industrial problems and readers familiar with developments in America will consider the following statement on page 4 of the Introduction to be overcautious: "Its application in the wider field of the chemical industry is still a matter of some doubt, but in view of its striking success in other fields, it is felt that the possibility of its application to chemical plants should receive careful consideration."

The book, as it stands, should be most useful to the industrial investigator. The intention of the authors, stated in the preface, to keep the handbook up to date, will furnish an opportunity for the critical editorial overhauling which is needed to bring this work to a uniformly high level of excellence. A sequel on the planning of experiments is contemplated.

The typography is exceptionally good. Misprints are rare and unimportant.

REVIEW BY E. L. LEHMANN
Assistant Professor of Mathematics
University of California, Berkeley, California

AUTHORS of any book such as the present manual who, without assuming any previous statistical knowledge on the part of their readers, want to give an introduction to intelligent use of statistical methods are concerned with three different aspects of statistics. (*a*) They must explain the basic concepts such as probability, expectation, statistical hypothesis, power of a

[1] Dudding, B. P., and Jennett, W. J. *Quality Control Charts*. British Standard 600R: 1942. London: British Standards Institution, 1942. P. 89.

test, confidence intervals, etc. (*b*) They must describe the most important statistical procedures, state the conditions under which these procedures are known to be valid, and discuss some of the computational problems that arise. (*c*) They must illustrate the methods with representative examples from the field of application with which they are concerned.

In the present volume, as in most other books of this kind, (*b*) and (*c*) are emphasized at the expense of (*a*) and even in the discussion of particular procedures the underlying assumptions are frequently indicated only rather vaguely. This seems regrettable since, as Professor Wolfowitz pointed out in a recent review in this JOURNAL (41: 127–8 Mr '46), "misapplication of statistical methods is more likely to be serious than the use of inefficient computing techniques." In the discussion of hypothesis testing, for example, no mention is made of possible alternatives or, therefore, of power. As a result the assumptions underlying a test are never quite clear. For instance, when the authors explain their recommendation of a two-tailed F-test in analysis of variance, the class of alternatives which they have in mind seems to change during the discussion.

The material covered in the book is fairly conventional. Two introductory chapters on frequency distributions, and averages and measures of dispersion are followed by a discussion of tests of significance (t-test and F-test—the authors make the common slip of using a one-tailed F-table for a two-tailed test without adjusting the level of significance). Then come the two main chapters on analysis of variance, and regression and correlation, and chapters on frequency data and contingency tables, sampling, and control charts. At the end are a few pages on prediction and specification, which are mainly concerned with the Camp-Meidell and Tchebycheff inequalities.

There is a somewhat incomplete glossary of statistical terms and an unusually comprehensive collection of tables (area under normal curve, percentage points of χ^2, t and F; values of correlation coefficient for different levels of significance; limits of the expectation for binomial and Poisson distributions; and control chart limits for average and range). A nice feature of the book is that the methods are illustrated throughout with many interesting examples from the chemical industry.

The authors state that they intend to keep the handbook up to date and indicate that they hope to include some sequential techniques in a later edition. It would perhaps be useful also to include some nonparametric methods.

Selected Techniques of Statistical Analysis: For Scientific and Industrial Research and Production and Management Engineering. By the Statistical Research Group, Columbia University, Applied Mathematics Panel, Office of Scientific Research and Development. Edited by *Churchill Eisenhart* (Chief, Statistical Engineering Laboratory, National Applied Mathematics Laboratories, National Bureau of Standards, Washington, D. C.); *Millard W. Hastay* (Research Staff, National Bureau of Economic Research, New York 23, N. Y.); and *W. Allen Wallis* (Professor of Statistics and Business Economics, University of

Chicago, Chicago 37, Ill.). New York 18: McGraw-Hill Book Co., Inc. (330 West 42nd St.), 1947. Pp. xiv, 473. $6.00. (London W.C. 2: McGraw-Hill Publishing Co., Ltd. [Aldwych House, Aldwych], 1948. 36s.)

REVIEW BY HENRY SCHEFFÉ
Associate Professor of Engineering, University of California
Los Angeles, California

THIS book consists of 17 quite unrelated chapters dealing with various problems which arose in projects undertaken by SRGC. Almost anyone confronted with practical problems calling for statistical methods may be assured of finding in this book material of value to him, providing he has some background of statistical knowledge, perhaps the amount acquired in two year courses in statistics. A very thorough index of 43 pages of fine print increases the usefulness of the book. It would not be suitable for a textbook for various reasons, including the specialization of the subject matter.

Since a summary of the book by chapters would take too much room here we shall list the titles and number of pages for each chapter, and add a few remarks. Part I, Industrial Statistics, consists of chapters 1 through 6; Part II, Planning Experiments, of chapters 7 through 13; and Part III, Techniques and Tables, of chapters 14 through 17. The chapters are:

1. Use of variables in acceptance inspection for per cent defective (92 pp.).
2. Tolerance limits for normal distributions (16 pp.).
3. Multivariate quality control, illustrated by the air testing of sample bombsights (74 pp.).
4. Effects of rounding or grouping data (40 pp.).
5. Acceptance inspection when lot quality is measured by the range (10 pp.).
6. Variability of amount of inspection for double, multiple, and sequential sampling (10 pp.).
7. Planning and analyzing experiments for comparing two percentages (20 pp.).
8. Planning and interpreting experiments for comparing two standard deviations (52 pp.).
9. Utilization of limited experimental facilities when the cost of each measurement depends on its magnitude (10 pp.).
10. Experiments in which the independent variable is known only in terms of a probability distribution (10 pp.).
11. Planning an experiment for estimating the mean and standard deviation of a normal distribution from observations on the cumulative distribution (14 pp.).
12. Abandoning an experiment prior to completion (10 pp.).
13. Planning experiments seeking maxima (10 pp.).
14. Probability that sample means are in opposite order to population means (8 pp.).
15. Significance of the largest of a set of sample estimates of variance (12 pp.).
16. Inverse sine transformation of proportions (22 pp.).
17. Elements of sequential analysis (13 pp.).

The chapters were written by Albert H. Bowker, Churchill Eisenhart, Milton Friedman, Millard W. Hastay, Harold Hotelling, Edward Paulson, L. J. Savage, Herbert Solomon, and W. Allen Wallis. Some of the chapters are based on work credited to the following people not included in the above

list of writers: Kenneth J. Arnold, M. A. Girshick, Myra Levine, A. Wald, and J. Wolfowitz.

Among the nonuniform aspects of the chapters is the mathematical preparation assumed on the part of the reader: At one extreme we find 7 pages devoted to computational methods for the evaluation of $\bar{x}+ks$, where $\bar{x}$ is the sample mean, s, the sample standard deviation, and k, a given constant (this may be justified by the resistance to the use of s in certain industrial quarters), at the other extreme there is mention of a set of measure zero (in a "technical note"). Mostly the authors have conscientiously tried to meet the needs of the "practical man" for whom the book was written, and if he happens to be also a mathematician he may occasionally find somewhat painful* the verbal arguments where he would use symbolic. If unifying motifs are sought in this work, one is to be found in the wide use of the concept of the power function or operating characteristic of a statistical test; this occurs in chapters 1, 5, 6, 7, 8, 12, 14, and 17. The reviewer considers one of the main virtues of the book to be the influence it will have in making this concept a tool of the practitioner.

The book brings to the reader a large amount of material which has been available before only in periodicals. Not even thus available has been much of the content of chapter 3 on multivariate quality control. Aside from the novelty of applying the T^2-type of statistic to quality control problems, we note also the following innovations: The distance apart of the control lines on the control charts depends on the length of the base period from which they are calculated (the distance decreasing with increasing length). "Yardsticks" are introduced on the control charts which allow us to judge whether the difference of the ordinates of two points is statistically significant. Where the ratio rather than the difference is statistically appropriate, the yardstick device is retained by use of a logarithmic vertical scale.

It is clear that this group did some very fine statistical work. For their efforts in rescuing some of the results of permanent value from burial in the files the director of the group, Professor Wallis, and the other editors deserve congratulations.

The Design of Experiments, Fourth Edition, *Ronald A. Fisher* (Arthur Balfour Professor, University of Cambridge, Cambridge, England). Edinburgh 1, Scotland: Oliver and Boyd Ltd. (Tweeddale Court), 1947. Pp. xi, 240. 12*s*. 6*d*.

REVIEW BY A. E. BRANDT
Statistical Consultant, Office of Technical Director
Naval Ordnance Laboratory, Washington, D. C.

THE reviewer has enjoyed the opportunity to appraise the fourth edition of *The Design of Experiments* after a number of years of teaching and

* Also that the one formally stated theorem in the book (p. 409) is possibly false, the proof certainly is, and in the two applications to discrete distributions the conditions of the theorem are obviously not satisfied [condition (14)]. Among the errors in the proof is the unjustified equating of $E(XY)$ to $E(X)E(Y)$ in several places. A correct theorem of the sort attempted, namely, one that is easily and widely applicable, would be nice to have.

applying the principles of design presented. These principles were first received by the reviewer in a course of lectures previous to publication of the first edition. Though each previous edition has been read and studied, the reviewer was well rewarded for the time spent on the latest one.

Familiarity with the statistical procedures presented by the author in his *Statistical Methods for Research Workers* is taken for granted as a background for understanding this book. However, the pleasing literary style and nonmathematical treatment in all but the last chapter have led some persons who lack this background to expect great benefits from reading it. Those readers who complain that the book is hard to understand are admitting their own shortcomings.

The course of lectures given by the author previous to the publication of the first edition of this book was called *The Logic of Experimentation*. The first two chapters and, to a large extent, the third, are more aptly described by this title than by the title of the book. In the first chapter the author presents a convincing argument for his theory of inductive inference and his reasons for rejecting inverse probability. By means of his classic experiment relating to a lady's claim that she can determine by taste whether the milk or the tea has been placed in a cup first, the whole logical structure of critical experimentation, from the statement of the question to the conclusion to be drawn from the experimental results, is developed. The roles of randomization and repetition or replication and the necessity of their inclusion in a critical experiment are made clear.

The data on Zea mays from Darwin's historical experiment on growth rate and Galton's interpretation of them are used to show how fallacious uses of statistics lead to discordant results and as a warning against the manipulation of data. The effect of local control, such as pairing or grouping, on the sensitiveness of each individual observation is clearly shown. The general test of the hypothesis that two samples are random samples from identical populations presented in the last section of the third chapter merits greater study by statisticians who lack experience in experimentation in some field of science such as biology, medicine, or physics.

To give a clear understanding of the logic of experimentation presented in the first three chapters and its practical application, the author uses a number of examples in chapters 4 to 9 inclusive. In the fourth and fifth chapters, simple situations are presented which can be handled by means of randomized block or latin and higher square designs which are the simple or basic types. In the next four chapters, designs suitable for situations of greater and greater complexity are illustrated. The advantages of the factorial arrangement over a series of experiments in which but one factor is varied at a time are emphasized in chapter 6. The use of local controls such as confounding and partial confounding and of statistical control through concomitant measurements are dealt with rather fully in chapters 7, 8, and 9.

Chapter 10 on the generalization of the null hypothesis and fiducial probability contains much of interest to the practical experimenter but is es-

pecially important for theoretical statisticians. The final chapter on the measurement of information is especially commended for serious study, not because of the calculus involved, but because the principles in it are ignored so frequently.

Though the author has used the results of actual experiments to illustrate the requirements of sound and intelligible experimental designs and means of drawing valid conclusions from them, he has avoided producing a compendium of designs which have been used successfully in various fields of experimentation. It would seem, however, that there is a distinct need for such a book.

Das Harmoniegesetz der Statistik: Eine Untersuchung über die metrische Interdependenz der sozialen Erscheinungen. [The Law of Harmony in Statistics: An Investigation of the Metrical Interdependence of Social Phenomena.] *L. V. Furlan* (Professor at the University of Basel). Basel, Switzerland: Verlag fur Recht und Gesellschaft A.-G., 1946. Pp. xii, 504.

REVIEW BY HILDA GEIRINGER
Professor of Mathematics, Wheaton College
Norton, Massachusetts

THIS book presents an investigation on statistics rather than a statistical investigation. As the author asserts, the starting point is basically new. The purpose is to study great masses of statistical numbers without any consideration of their meaning but with respect to some "number-theoretical" properties. The result is a very definite statement concerning a very vaguely defined subject: Consider a mass of statistical numbers, "the way they are found in every statistical yearbook." They form together some kind of reflection of the social life of a group of people, of a nation, or of groups of nations. Such a mass of statistical numbers, no matter whether it consists of only a few or of very many numbers, represents a sample of the infinite mass of numbers which describe the social phenomenon, or rather some aspects of the social phenomenon. As an example a "collective" consisting of 1.19×10^4 numbers is considered (quoted from *Statist. Handbuch der Weltwirtschaft,* Berlin, 1936), containing data on various fields of economical life for eighty countries for the year 1933. (This collective is analyzed as a whole and also resolved into 80 partial collectives.) Another example contains an even larger mass of numbers, practically all numbers contained in the *Statist. Jahrbuch der Schweitz, 1912.* (In each of these and other "natural" collectives certain adjustments are made before the analysis of the material is started.)

The main problem and aim of this analysis is a very strange one. The first figure in a natural number is called its "indicant." If decimal fractions are considered, the first nonzero number is the "indicant." In a decadic system there are nine possible indicants. For each of these nine indicants, their relative frequency within the considered collective of numbers is computed.

The author's contention is that, quite independently of the meaning of these statistical numbers, if the "natural collective" is only large enough and somehow a random collective, there exists a striking regularity concerning these nine relative frequencies. (These nine frequencies form the "spectrum" of the natural collective.) The statement is that for a natural collective the relative frequency of the indicant i, $(i=1, 2, \ldots, 9)$ equals $\log(1+1/i)$. If this is so, i.e., if the relative frequency of the indicant (i) has this value, the spectrum is called the "harmonic spectrum," and this validity of the harmonic spectrum for natural collectives constitutes *the harmony law of statistics*. A particular consequence of this law is the fact that the indicant "one" appears with relative frequency $\log 2 = .30$; hence, it appears much more frequently than it would in a uniform distribution of the nine numbers. This is called "concentration on the one" and is considered as the most striking and the most important feature of the law. The frequency of the numbers $1, 2, \ldots, 9$ then decreases according to this law. This "harmony law" which forms the main result of the 500-page work may be stated once more: "The spectrum characteristic of natural collectives is the harmonic spectrum" (p. 443). Or, in the more specific form: "The indicant of a statistical number is a chance variable which assumes the values $i=1, 2, \ldots, 9$ with probability $\log(1+1/i)$" (p. 468).

As a theoretical background for these statements certain "artificial," i.e., mathematically defined, collectives (sequences) are considered with respect to their "spectra." Examples are: (a) the sequence of natural numbers $\{n\}$ (where $n=1, 2, \cdots$); (b) the sequence of the squares of natural numbers $\{n^2\}$ $(n=1, 2, \cdots)$, of the cubes, etc.; (c) the sequence a^n (where a is fixed and $(n=1, 2, \cdots)$; (d) the sequence of all rational numbers in the well known order $\frac{1}{n}, \frac{2}{n-1}, \frac{3}{n-2}, \cdots, \frac{n}{1}$ $(n=1, 2, \cdots)$ (fractions with a common divisor of numerator and denominator are discarded). For any such sequence the relative frequency of the indicant i within the first n elements is computed, and the limit of this relative frequency as $n \to \infty$ is considered. It is easily seen that for the first two examples such a limit does not exist. (Such "collectives" are termed "erratic.") On the other hand it is shown that for (d), the limit distribution of the indicants is the harmonic distribution; i.e., the relative frequency of the indicant i equals $\log\left(1+\frac{1}{i}\right)$.

It is not the reviewer's business to express an opinion as to whether such facts are of mathematical interest from the point of view of pure mathematics. In the reviewer's opinion the main problem consists obviously in constructing a connection between them and the harmonic property of natural collectives, a connection which would allow consideration of a certain theoretical scheme as a "model" of a natural collective. Such a "model" is, in fact, indicated, namely the "artificial collective" (c): $\{a^n\}$

($n=1, 2, \cdots$), which shows "approximately" the harmonic spectrum (p. 303). "The exponential function which expresses the law of natural growth forms a scheme of the many small causes which act by way of multiplication, being unobservable as long as they are isolated but becoming manifest by means of the spectra of natural collectives" (p. 503, translated and condensed). "In this way the natural collective becomes an instrument which reveals certain properties of the social phenomenon which could not have been found in any other way" (p. 502). Thus, on the basis of the interpretation of natural collectives by means of the sequence $\{a^n\}$ a theory of the natural collectives is somehow attempted. This theory is, to say the least, extremely vague.

It may well be that a mathematician is not in the position to do justice to a work belonging to the field of social sciences. With this reservation a few comments may be allowed. It is hard to decide whether it is possible to give precise definitions to the main concepts introduced in the statement of the harmony law, as "natural collective," "statistical number," etc. There seems to be little point in making mathematically precise statements concerning incompletely and vaguely defined subjects. If such definitions are given the questions will remain: (*a*) whether this law expresses a theoretically consistent statement, and (*b*) whether in its application to reality it states a significant feature of reality, i.e. of the social reality behind the natural collectives. This seems to the reviewer rather improbable, certainly so far as mathematically precise statements are concerned.

If we look in a purely empirical way at the examples in Furlan's book (e.g., p. 22) or if we examine, on our own, some statistics in yearbooks, it actually seemed to the reviewer as if there existed a certain vague "concentration on the one" as first figure. But here again some rather obvious remarks force themselves upon us, e.g., that this concentration may be destroyed by a change of unit ("100 square miles" instead of "100 hectares," or "per 5000 people" instead of "per 1000 people"), or that statistical numbers have upper and lower bounds (think of the possible numbers of children in a family, to quote a trivial example) which influence the indicants. If we consider this question of unit a little closer it seems that the "harmony law" ought to be independent against multiplication by a constant in order to be independent of the unit of measurement. This would imply the following theoretical statement: Consider an "artificial collective," i.e., a sequence of numbers where the indicant, i, appears with a limit of relative frequency equal to $\log(i+1)-\log i$, as postulated by the harmony law. If in such a sequence each number is multiplied by one and the same constant the relative frequency of the nine indicants should remain unaltered. This however is certainly not true. As a simple conterexample consider a sequence consisting only of the nine numbers, $1, 2, \cdots, 9$ which are each written down so often that the number i occurs with the frequency (or more precisely with the limit of relative frequency) equal to $\log(1+1/i)$. Now form a new sequence by multiplying each number in the original sequence by the constant 2. This new se-

quence will consist of the numbers 2, 4, 6, · · · , 18 where the "2" will appear with frequency log 2, the "4" with frequency log $\frac{1}{2}$, · · · the "18" with frequency log 10/9. In this new sequence the indicant "1" appears in 10, 12, 14, 16, 18 with frequency equal to log 6/5+log 7/6+ · · · +log 10/9=log (6/5·7/6·8/7·9/8·10/9)=log 2, hence in fact again with frequency log 2. But, e.g., the indicants 3, and 5, and 7, and 9 will each have frequency zero instead of the frequencies postulated by the harmony law which thus appears to be completely destroyed by this operation.* Undoubtedly such obvious remarks must have occurred to the author. They apply, in this form, of course to artificial, i.e., mathematical collectives only. A certain empirical regularity like the concentration on the one may be observable on "natural collectives" under certain specified conditions. However, an empirical observation, even if correct and fairly general hardly constitutes a convincing basis for a far reaching general mathematical statement. And the theoretical foundation by means of a mathematical model (see our remarks concerning $\{a^n\}$ can hardly be considered at this stage as much more than an aperçu.

An impeding feature of the book is the lack of an index. This is particularly disagreeable since so many new and unusual words and notations are used. Otherwise the work is certainly extremely well written and very original. It remains to be seen whether this whole problem of the relative frequency of the indicants of statistical numbers reveals—by means of a suitable interpretation—aspects of social phenomena which are of interest to social scientists.

Fundamentals of Statistics. *Truman Lee Kelley* (Professor of Education, Harvard University, Cambridge, Mass.). Cambridge 38, Mass.: Harvard University Press (38 Quincy St.), 1947. Pp. xvi, 755. Lithotyped. $10.00. (London E.C. 4: Oxford University Press [Amen House, Warwick Square], 1948. 55*s*.)

REVIEW BY CYRIL BURT
Professor of Psychology, University College
University of London, London, England

THIS book is more than a revised and expanded edition of Kelley's well-known *Statistical Method* (Macmillan Co., 1923). But it preserves many of the admirable features of that excellent work. In his approach, as the author pointed out at the time, his "greatest inspiration" had come from Karl Pearson; and he found "the English School had been most contributive." The present volume shows how Pearson's general conception of statistics and the numerous contributions that he made on fundamental problems

* It is obvious that the same conterexample holds if instead of the logarithmic frequencies, postulated by the harmony law, any other nine frequencies, $p_1, \ldots p_9$ with sum equal to one, are assumed. It seems to the reviewer that from a purely mathematical point of view, it is, the concept of the "indicant" which is open to objections rather than the particular frequency distribution considered by the author.

may still serve as the foundation on which the newer additions of more recent years may be superposed.

The chief alteration in the new volume lies in the greater attention paid to the logical principles underlying statistical reasoning. Statistical procedures are presented, not as a "bag of useful tricks and dodges," but as a mode of reasoning about complex problems. This point of view is developed clearly and convincingly in the opening chapter.

The chapters that follow, like those of the earlier book, deal in order with statistical series and statistical tables, graphic methods in two and three dimensions, and the "stable features of phenomena" (a topic that was only briefly touched upon in the earlier volume). This leads to the methods of measuring central tendency and variability, and the properties of the normal distribution. A fresh chapter is then inserted on the statistics of attributes; the use of chi-square and the variance ratio is introduced in this connection. We then proceed, much as before, to the problems of correlation, regression, and multiple correlation. The latter, it may be remembered, Kelley dealt with on Pearsonian lines in terms of determinants. It is therefore natural for him now to introduce the uses of matrix methods in this connection.

The original chapter on "sundry statistical problems and procedures" is considerably enlarged. Some of the newer topics are those of periodicity, lead and lag in time series, direct and inverse interpolation, the variance errors of different statistics, and (another welcome addition) sequential analysis.

The last chapter is entitled "mathematics: the mentor of statistical ingenuity." Its main purpose is to provide, in compact form, a mathematical background for the experimentalist, so that he can equip himself with a knowledge of the particular functions that are most likely to be suggestive of phenomenological relations. Accordingly, in addition to a discussion of the point binomial, the Poisson distribution, and the hypergeometric series, we have sections on the Fourier series, factorials, growth curves, matrix algebra, the determinantal solution of simultaneous equations, and other topics. The book concludes with a useful set of statistical tables, a mathematical background test, and lists of symbols and common mathematical terms (with definitions), and a key to formulae.

It will be seen that, while the book includes nearly all the stock topics, it also contains much useful matter that is not to be found in the usual type of statistical textbook. As the author points out, statistics has developed so rapidly during the last twenty-five years that it would be impossible for anything short of an encyclopaedia to cover the entire ground. It is therefore easy for the critic to point out omissions. The psychologist will probably regret that more attention has not been given to the analysis of variance and to factor analysis. In the discussion of the logical basis it is perhaps disappointing that more space has not been given to the theory of probability and its relevance to tests of significance, and particularly to the latest developments in the theory of estimation.

REVIEW BY HORACE W. NORTON
*U. S. Atomic Energy Commission**
Oak Ridge, Tennessee

FUNDAMENTALS *of Statistics* is an elementary textbook written by a well-known psychologist. Although the early chapters usually employ little mathematics, the later chapters use a fair amount of algebra (especially in treating determinants and matrices), and there is an occasional appeal to the calculus. The book is addressed to no specified class of readers, but it is fair to say that in flavor, language, examples, references, and point of view, it is definitely of the social sciences. Its object is clearly stated in the following quotation:

> The usual elementary text in statistics seeks to acquaint students with the techniques necessary to obtain . . . averages, measures of variability [etc.]. . . . This text considers these things as a secondary aim, the primary aim being to acquaint the student with the logic back of these procedures. It is believed to be more important . . . to understand why a mean is employed than to know the steps for calculating it . . . (p. 57).

As a text, it should be regarded as of more nearly 500 pages than 750, roughly 200 pages being given to miscellaneous aids, such as mathematics, references, tables, and indexes. There are a few problems, and much emphasis on vocabulary.

The first two chapters are introductory, exhibiting to the reader the meaning, the scope, and the uses of statistics. Thirty pages are given to a discussion of the mathematical background needed for statistical reading, quoting the Social Science Research Council report (1932) and including brief explanation or comment on a number of elementary mathematical topics. This is unusual by itself, and it is therefore especially noteworthy that there is also a mathematical background test with instructions and scoring key, enabling the student to assess objectively his readiness to read this book. The next nine chapters (275 pages) are conventional in subject matter, treating statistical tables, graphics, the notion of stable features of variable phenomena, measures of variability and of central tendency, the normal distribution, attributes, regression and correlation, and multiple regression. Chapter 13 is a miscellany, including time series and periodicity, methods of arithmetic such as machine extraction of roots, and sequential analysis. Chapter 14 is chiefly mathematics, but some statistical topics occur, such as moments of the binomial and Poisson distributions, and some transformations. The fifteenth and last chapter gives a few mathematical and statistical tables and a considerable list of references to such tables. Appendix A gives the mathematical background test already mentioned. Appendix B lists symbols and terms, formulas given in the text, and references.

Because the author has not designated a specific group of readers as his

* This review should not be construed as representing the official position of the U. S. Atomic Energy Commission.

target, critical evaluation is not easy. Moments are emphasized, and there is a lengthy discussion of the correlation coefficient, its deficiencies, corrections, interpretation, and uses; and of matrices and determinants; solution of linear equations; and partial and multiple regression. The analysis of variance is mentioned a number of times but without systematic treatment. There is nothing on independent comparisons or single degrees of freedom, no mention of components of variance, no analysis of variance tables, and no worked example. Analysis of covariance is not mentioned, and experimental design receives only a half page. All this may be accepted by students and teachers in the social sciences, though its adequacy for their needs is surely doubtful. However this may be, it certainly will be unacceptable to most, perhaps all, other beginning students of statistics, though doubtless many will realize this only when they have ceased to be beginners.

The lack of systematic treatment of the analysis of variance and covariance is the more difficult to understand because it is so widely regarded not only as fundamentally important to applied statistics but also as so helpful to the author's stated primary objective of emphasizing the logic and understanding of statistics. Likewise, systematic treatment of experimental design would have been helpful to the same end, revealing as it does an entirely different aspect of the statistical whole, even if it were kept to only a few pages. Nevertheless, the author pursues his objective with some success, chiefly in the early chapters. He has done well, for example, in formulating some of the quasi-axioms of statistical thought, which can be so helpful to the tyro who remembers them, such as:

> The reader should develop the habit of appreciating that *an answer in terms of probability exists for every observed difference between two statistical measures*, even though he does not have the necessary ability, or perhaps data, to arrive at a precise statement of it. Just as the error in statistical statements is ubiquitous, so should be the awareness by the student of this fact (p. 78).

On the other hand, it should not be supposed that misleading and erroneous statements have been entirely avoided, as the following examples show. It is asserted (p. 73, using hypothetical temperature forecasts!) that the error is independent of the prediction "as it universally must be." There may be a definition of "error" which makes this statement true, but it need not be and usually will not be for real data analogous to the example. After a test for normality resulting in rejection of the hypothesis, we read, "knowing that successive daily maximum temperatures at a single spot are correlated and not independent, we have a ready explanation in that our items are not independent random samplings" (pp. 304–5), but of course this has nothing to do with the normality of distribution of either the sample or the parent population. A graph of the chi-square distribution for one degree of freedom (p. 285) shows an inflection where chi-square is about 0.13. Of t it is said that "It is the distribution of [means, differences of means, and of regression coefficients] . . . computed from small samples drawn from a parent normal population" (p. 286), obscuring the nature of this revolutionary statistic.

We are told (p. 226) that the expected value of the standard deviation is the square root of the expected value of the variance. There is the old error of "explaining" kurtosis simply in terms of the relative frequencies near the mean and in the tails (p. 242).

In addition to faulty statements, there are errors in formulae and tables, many of which are probably typographical, though none the less consequential to the student. For example, formulae for Fisher's fourth k statistic appear on pages 215 and 247, neither being correct, and a few errors were noted in the table (pp. 640–52) of normal probability functions.

The list of tables of statistical functions (including the usual mathematical tables) is excellent, each table being described as to content. It was a pleasure to the reviewer to find himself correctly credited for the table of the 20 per cent level of the z and variance ratio distributions. However, this pleasure was immediately mixed by inclusion of the 0.1 per cent table which of course is due to C. G. Colcord and L. S. Deming. The list of references, to both books and papers, would be much increased in value by elimination of such entries as Peters and Van Voorhis, *Statistical Procedures and Their Mathematical Bases* or, better still, by furnishing critical comments for each entry.

The notation seems unnecessarily cumbersome, and the terminology is sometimes unfamiliar, not to say strange. For example, there is frequent use of a symbol with both a bar and a tilde, the value found in a table is called the "consequent," as the value used to enter the table is called an "argument," and the reviewer is still wondering what the "xlnt" correlation chart is. This may be the appropriate place to remark that both "random" and "bias" are often used but never defined.

The author indulges in occasional flights of fancy as to subject matter or phrasing which serve to enliven the text, as when, in discussing significant figures, he considers a palsied man using a meter stick to measure a live angleworm.

The typography leaves much to be desired, both in appearance and accuracy, the spacing of lines, words, and letters being irregular, and typographical errors being frequent, especially in names. The index suffers from numerous errors of omission and commission, and additional cross-referencing would be helpful.

Mathematics of Statistics, Part One, Second Edition. *John F. Kenney* (Assistant Professor of Mathematics, University of Wisconsin at Milwaukee). New York 3: D. Van Nostrand Company, Inc. (250 Fourth Avenue), 1947. Pp. xii, 260. $3.75. (London W.C. 2: Macmillan & Co. Ltd. [10 St. Martin's St., Leicester Square]. 21*s*.) *Two reviews follow:*

REVIEW BY HOWARD LEVENE
Lecturer in Mathematical Statistics
Columbia University, New York, N. Y.

THIS is a slight revision, with minor clarifications and additions, of the first (1939) edition. The book deals almost exclusively with descriptive statistics, and requires no mathematics beyond high school algebra.

The first hundred pages deal with the mechanics and graphical representation of frequency distributions, the usual measures of central tendency and dispersion, and higher moments. These are followed by chapters on the normal curve, curve fitting, and correlation. The use of tables of the normal curve for curve fitting and graduation by areas is discussed. There are sections on fitting straight lines and second degree parabolas. An exponential trend is fitted by using the method of least squares on the logarithms, with a footnote warning that this is not the same as a least squares fit to the exponential itself. Two pages are devoted to fitting the Gompertz curve without an explanation of its use, while the logistic and Makeham's curve are merely mentioned and references given.

The summation notation is introduced early and used consistently throughout the text. This lends much greater clarity to the extensive discussion and proofs of various moment relations than is usual in a text on this level. For example, for k sets of variates combined into one set, the author obtains the relation

$$\sigma^2 = \frac{1}{N}\sum n_i\sigma_i^2 + \sigma^2_{\bar{x}_i}$$

where

$$\sigma^2 = \frac{1}{N}\sum_1^N (x_\alpha - \bar{x})^2,$$

and states that this will prove important in the analysis of variance.

This book is clear and well written and should be an excellent text for a course dealing with the material it covers. However, the reviewer questions the purposes served by such a course. The word "probability" appears for the first time on page 114 and is occasionally used thereafter, but a discussion of the concept is deferred to Part II (not here reviewed), and the notion of sampling and sampling error appears only in vague form in some of the discussions. The same letter is used throughout for both parameter and statistic. Sampling of attributes is not mentioned, presumably since a coherent discussion of it would require probability theory. Because of these and other deficiencies the book is not suitable for a terminal course in statistics. Students intending to go on to a "practical" course in statistics do not require such a thorough algebraic foundation, while those intending to take further courses in mathematical statistics might do better to devote their time to pure mathematics, so as to be prepared for an introductory statistics course using the calculus and probability theory. With the difficulty of finding time in the curriculum for all the subjects a student should study, it is doubtful if time should be devoted to material which must be largely repeated, in somewhat different form, in later courses.

Both the virtues and the defects of the book are well shown in the chapter on correlation. There is a good discussion of the nature of normal correlation. The normal bivariate surface is treated briefly, but the major mathematical treatment in this chapter is in terms of an existing finite sample.

Conclusions are drawn from this about the predictive value and the amount of relationship implied by a given correlation coefficient. While there is verbal reference to some vague "large" set of values or population, it would seem that this treatment would be confusing. Blakeman's criterion of linearity of regression is given, with a warning that it is not reliable and a reference to Part II for the analysis of variance test. The example on page 220 for which Blakeman's criterion is said to break down is poorly chosen, since the analysis of variance test is not applicable. Blakeman's criterion is the only significance test mentioned in the present volume, there being no mention that a criterion for existence of regression would be desirable.

There are a few minor errors, misprints and passages to which exception could be taken, but there is only one that the reviewer feels he should mention. On pages 147 ff. it is recommended that when two variables have a linear relationship and both are subject to errors, the sum of squares of perpendicular deviations from the fitted line should be minimized. This method is not invariant under change of scale of measurement. The problem of the determination of structural relationships is an important one, but requires a much more complete specification of the underlying chance mechanism, and even then cannot always be solved.

REVIEW BY RONALD W. SHEPHARD
Assistant Professor of Mathematics and Economics
New York University, New York, N. Y.

GENERALLY speaking this revision is not a substantial alteration of the previous edition—an elementary textbook on descriptive statistics, much used and a pedagogically sound treatment. The topics covered, their order and method of presentation, and the illustrative material are identical. The altered format of the revision results in a change of page numbers for corresponding textual material, which is a nuisance for those wishing to use it along with the earlier edition, but it has produced a better integration of figures and tables with related discussion. Still, the paragraph numbers are identical and one may avoid the changed page numbering.

It is difficult to find any changes in the textual discussion, and most students would use the revision along with the earlier edition without recognizing any differences. A new paragraph on variables and constants is added at the beginning of Chapter 1, and a not very useful paragraph on probability in Chapter 6. In the chapter on curve fitting a new paragraph is added dealing with logarithmic coordinate paper, and immediately preceding this Kenney has introduced a useful discussion of graphical determination of an exponential function with two new illustrative figures which change the ensuing numbering. Otherwise the alterations of test consist in a sentence here and there. The tables in the revision and earlier edition are the same.

One would naturally expect substantial change and improvement of student exercises, but this has not been provided. The alterations consist

only in the addition of a few new exercises and constitute a disturbing deficiency of the revision for those looking for a new edition of this book.

More fundamentally, this revision is an example of a practice of publishers to rush into new editions at this time without having ready a thoroughgoing rewrite of their popular textbooks, resulting in little or no incorporation of new treatments and new applications of methods which have occurred during the war years. This is not to be taken as a criticism of Mr. Kenney's general method of treatment, which the reviewer has found to be a relief from the usual elementary textbook on descriptive statistics in the social sciences.

Descriptive and Sampling Statistics. *John Gray Peatman* (Associate Dean and Associate Professor of Psychology, City College of New York, New York, N. Y.) New York 16: Harper & Brothers (49 East 33rd St.), 1947. Pp. xviii, 577. $4.50.

REVIEW BY LEON FESTINGER
Research Center for Group Dynamics
Massachusetts Institute of Technology, Cambridge, Massachusetts

IN THE past two years there has been somewhat of a spurt in the publication of books on statistics, the reason for which is not too evident. Certainly many of the books on statistics which were published before the war had their little inadequacies, but the books published since the war seem on the whole to possess the same inadequacies or else to substitute deficiencies of their own for those of their predecessors.

The motivation for the publication of these new books does not seem to be the presentation of new material either. While the war period did produce a number of new developments in statistical methods, these are rarely included in the new books which appear. Perhaps the explanation lies in the differences of opinion, which no doubt exist, as to what to emphasize and how to teach statistics. The implication is certainly present, however, that a new book on statistics should be evaluated not only in terms of what it includes and how it presents its material but also in terms of the possible justification for using this new book rather than one which already happens to repose on the library shelf.

The title of the book under consideration, *Descriptive and Sampling Statistics*, correctly implies that the book treats separately the use of statistics for purely descriptive purposes—to summarize data—and the use of statistics as a tool for testing hypotheses concerning population parameters. These two sections of the book are of very different quality and must be evaluated separately.

The first part of the book, entitled "Descriptive Statistics" is, with respect to the content it covers, rather comprehensive. There is the customary array of frequency distributions, graphical representations, percentiles, means, medians, measures of deviation, and the like. There are some differences, however, between this and other similar texts. For one thing the discrepan-

cies in terminology among different writers on statistics have led the present author to seek to change this terminology at various places. We thus find terms like "non-variable data" defined to mean what is usually meant by discrete data which does not necessarily form a scale, and we find the invention of new words such as vigintile, tercile deviation, and quintiserial correlation. Another difference which we find in this book is the somewhat heavier emphasis on the use of deciles and percentiles (which the author prefers to call centiles) than one usually finds. With this greater emphasis the author is able to point out quite clearly how the median is simply a particularly important centile value, and how the quartile deviation is one of a class of such deviational measures which might be computed.

In the preface to the book the author explains that he has "attempted a balanced presentation that will teach the student not only how to compute a statistical measure but when to use a particular technique and how to interpret a result." The degree to which this balance has been achieved, however, is open to some question. There is no doubt that a student who has carefully digested the material in the book will be able to compute the various measures which are presented. There are detailed steps for computation and numbers of illustrations of such computation. There is some doubt, however, whether this student will have an adequate enough understanding of what he is doing and why to insure the use of the proper measure where alternatives are available, and to insure adequate interpretation. In most places the book goes quite thoroughly into what to do but skims very lightly over the "why." For example, the reader of the book is told to correct a phi coefficient between two dichotomized variables by dividing by .637, but nowhere is there any mention of the reason for such a correction, the assumptions underlying this correction, or the conditions under which the correction should or should not be used.

The second section of the book, entitled "Sampling and Analytical Statistics," is of quite different character from similar sections in other textbooks. Some of these differences are commendable while others are rather unfortunate.

The present book is one of the very few which have attempted to include a discussion of sampling problems in public opinion research. The discussion is relatively brief, occupying only fourteen pages, but nevertheless does present the major techniques such as stratification, area sampling, and quota sampling. There is perhaps not sufficient critical evaluation of these techniques and not sufficient discussion of the problems of each. There is also, unfortunately, no presentation of sampling error formulae for dealing with these various kinds of samples.

Another feature of the book is a short but relatively good discussion of discrete probability distributions. This material, generally omitted from elementary statistics texts, includes important and useful statistical tools. This section, in the book under consideration, covers the theorems dealing

with the addition and multiplication of probabilities and discusses the binomial expansion in detail.

Unfortunately, the remainder of the second part of the book is of rather inferior quality. The presentation consists almost exclusively of tests which might be justifiable when the number of cases in the sample is sufficiently large so that anything and everything distributes itself normally. The only exception made is for correlation coefficients where Fisher's z transformation is given. The t test is dismissed in one page with the idea that it is not very useful in psychology and the social sciences; the F ratio for significance of difference between variances is never mentioned; the techniques of analysis of variance are entirely omitted and the discussion of the chi-square test is rather poor. It may be that this selection of material was determined by the idea that this was to be an elementary text, but the reviewer finds difficulty understanding why outdated and inferior procedures are more elementary than newer and more adequate procedures.

It would undoubtedly be extremely difficult to write a textbook on statistics which was suitable for use in an elementary course without having some inaccuracies in the presentation. In the present textbook, however, there seem to be many inaccuracies which could be easily removed. We will list two of these doubtful statements so as to give the reader an idea of their nature.

> Thus, for a series of samples of less than 25 or 30 cases each, the form of the sampling distribution of any statistic derived therefrom will skew more and more from the normal probability curve, the smaller the size of the sample (p. 324).
>
> . . . *chance errors* are errors that are just as likely to occur as not to occur; hence they are equiprobable (p. 332).

Whether or not readers will find *Descriptive and Sampling Statistics* a worthwhile text to use will undoubtedly depend largely upon their personal preferences as to what should be taught in an elementary statistics course and how it should be taught.

REVIEW BY FREDERICK F. STEPHAN
Director of Study of Education and Professor of Statistics
Princeton University, Princeton, N. J.

How should a textbook be appraised—in terms of its competition or in terms of the needs it should satisfy? Compared with other texts in current use, this book has several commendable features that will appeal to some teachers and students. For the most part, these features consist of interesting discussions of the meaning and applications of statistical methods in psychological testing and public opinion polls. Among them one may mention an interesting sketch of the historical background of statistics and several passages in the sections on sampling, test validity, and correlation that reveal considerable skill and ingenuity in presentation. However, there is another standard by which a textbook may be judged, namely, its effec-

tiveness in remedying the defects of other books and bringing the teaching of the introductory course abreast of recent developments in basic theory and method. On this standard, the book does not appear to be satisfactory. To too great an extent it seems to be a rewriting of the content of familiar textbooks, principally Peters and Van Voorhis, and parts of Croxton and Cowden, and Yule and Kendall, rather than a fresh and modern formulation of the introductory course based on current research applications and methodological advances. It even falls behind these books (published some ten years earlier) in many respects. For example, the reviewer could not find as much as a casual reference to the analysis of variance.

Of course, there is plenty of room for disagreement about what is appropriate to a first course, even one of a year's duration, especially when it is addressed, as this is, to students with no college mathematics. Yet, if the author really expects to give students "the ability to select the most relevant statistical technique and to make the necessary computations with a minimum of error" (p. xvii), he could well include a discussion of such topics as the efficiency of estimating functions and the power functions of statistical tests, at least to the extent of providing a clear statement of the problem to be solved in each case. Certainly these concepts are fundamental to an understanding of the logic that underlies the application of statistical methods and they can be presented simply enough to students who can understand the great variety of correlation methods that are discussed in great detail.

While the book is intended for research workers in the social and biological sciences, it neglects important parts of these fields and emphasizes psychological and educational testing and public opinion polling. It slights psychological experimentation, genetics, and other major regions of the application of statistical method to research in these fields.

The book is divided almost equally between "descriptive" statistics and "sampling and analytical statistics." As a consequence the normal distribution and correlation are discussed extensively before sampling and probability. This separation is harmful, in the opinion of the reviewer, for it obscures the extent to which statistical theory has been unified.

The topics that are covered include: categorical data, proportions, ratios, index numbers, percentages, correlation of categorical data, frequency distributions, centiles, mean, standard deviation, the normal curve, correlation, sampling, probability, tests of significance, chi square, prediction from correlations, evaluation of psychological tests, and cluster and factor analysis.

The author took pains to avoid sacrificing accuracy in attempting to write clearly but there are a number of places at which statements are confused or incorrect when read literally. A few examples will be cited:

An IBM card is illustrated and described as having ten positions in each column (p. 38). No mention is made of the x and 12 or v positions.

"The arithmetic mean . . . and the standard deviation . . . The method of their computation is essentially algebraic and is often described as the method of moments" (p. 150).

"The normal distribution . . . is a unimodal distribution with the greatest number of frequencies at the mean" (p. 175). The discussion of the normal distribution is inadequate; ordinates are given as probabilities (p. 184).

If a test of significance does not indicate that the difference is significantly greater than zero, "we can be confident that no real differences exist in the characteristics compared" (p. 401).

There is a meager one-page bibliography. It does not include such books as Snedecor or Lindquist, which are needed to cover important fields that this book slights. The appearance of books like this emphasizes the importance of more effective communication between statisticians in various fields and improvement in the teaching of statistics, two functions in which the American Statistical Association should be increasingly active.

Fundamentals of Statistics. *J. B. Scarborough* (Professor of Mathematics United States Naval Academy, Annapolis, Md.), and *R. W. Wagner* (Assistant Professor of Mathematics, Oberlin College, Oberlin, Ohio). Boston 17, Mass.: Ginn and Co. (Statler Building), 1948. Pp. vii, 145. $2.50.

REVIEW BY LEO AROIAN
Assistant Professor of Mathematics, Hunter College
New York 21, New York

THE appearance of a new text on statistics is always a source of pleasant anticipation for the harassed teacher searching hopefully for the ideal text. The present text, intended for engineering and science students, found its birth in the need for a short course of twenty lessons at the United States Naval Academy. An elementary knowledge of calculus is assumed. The book is remarkably brief. Simplicity and clarity are stressed throughout. The eight chapter headings and appendices are representation of data, averages, measures of dispersion, comparison of distributions, correlation, probability functions, the normal curve and a generalization, sampling, one appendix on probability, a second one on the tables of the normal curve and the second, third, and fourth derivatives of it, a third one listing nine texts on statistics recommended for collateral reading, and five pages of answers to problems.

The originality in conception of the book must be highly praised. Undoubtedly it will be extensively imitated. The mathematics carries the burden of the explanations. Very few words are wasted. The authors exhibit considerable ingenuity in the development of the topics and are at their best in descriptive statistics, excellent in their treatment of probability and sampling once their point of view is granted. They have not been afraid to drop many topics considered sacrosanct such as Sheppard's corrections. Many problems illustrate the main points of the text. A nice balance is maintained between theory and numerical computation. Yet there is a greater nod to the past than to the future.

Fundamentally the reviewer disagrees with the authors' philosophy of

testing hypotheses. Some intuitive explanation of the modern theories of probability as developed by von Mises and Kolmogoroff would be desirable. In many places too little explanation is given. Only two-sided tests of hypotheses are mentioned and needless to say nothing concerning Type II errors, the error of accepting a false hypothesis. For sample means, N large, this is a simple matter. More serious is the confusion of two entirely different problems: "Given a probability function, all parameters known including the population mean, what is the probability of obtaining a sample mean $\bar{x}$ greater than x_0?" with the problem "A sample of N variates is drawn, N large, from an unknown population (possibly of known functional form), a statistic is calculated say the sample mean, what can be said of the population mean?" On page 106 the statement is made: "$\cdots N_{pop} = N_{samp} \pm \frac{\sigma_{samp}}{\sqrt{n-1}}$. This is interpreted to mean that the probability is about 2/3 that the mean of the population is within the range $M_{samp} - \sigma_M$ to $M_{samp} + \sigma_M$. "(!) Actually after a sample is drawn the probability that the population mean should lie in such an interval is zero or one. What is needed here is the theory of confidence intervals, certainly a fundamental topic.

Some important topics omitted are significant figures, the range, J-shaped curves, the chi-square test of goodness of fit, analysis of variance, control charts, and small sampling theory. Topics which might readily have been omitted are the quartile deviation, the coefficient of curvilinear correlation, and the coefficient of rank correlation. On page 24, $\sigma_x = a\sigma_y$ should read $\sigma_x = |a|\sigma_y$. The common error concerning α_4, the measure of kurtosis, occurs on page 33. The attempt is made to justify their statements by use of a Gram-Charlier Type A of three terms including α_4. As Cramer remarks in his recent text (p. 184, p. 230) an additional term $\frac{10\alpha_3^2}{6!}\phi^{(6)}(x)$ is required; consequently the authors' remark is not accurate even for a Gram-Charlier series in which the terms are grouped according to the same order of magnitude. Naturally in some particular cases $\alpha_4 > 3$ is associated with a high peak at the mean. The final discussion on the limitations of the coefficient of correlation is good.

A word might be said about notation. The letter x is used to represent the original variable, a variable with mean zero, and a variable with mean zero and standard deviation 1. The mean is usually written as M whether referring to a sample mean or a population mean, although occasionally a distinction is made as on page 105. The same remark applies to the standard deviation and the measures of skewness and of kurtosis.

It is a pity that in the section on collateral reading no mention is made of the recent books by H. Cramer, H. A. Freeman, P. Hoel, A. Wald, C. E. Weatherburn, and S. S. Wilks.

Practical Rules for Graphic Presentation of Business Statistics. *L. Edwin Smart* (Professor of Economics) and *Sam Arnold* (Assistant Professor in Business Research). (Ohio State University.) Columbus, Ohio: Bureau of Business Research, College of Commerce and Administration, Ohio State University, 1947. Pp. xii, 86. $2.00.

REVIEW BY RUDOLF MODLEY
President, Executive Research, Inc.
350 Fifth Ave., New York 1, N. Y.

THE authors of this small book have set themselves the task to write a simple "How to Do" book on the graphic presentation of statistics for businessmen and students. In going about their job they have set forth specific rules applying to all charts and rules applying only to some types of charts. Each text discussion is accompanied by one or more illustrations. The book deals with bar charts, pie charts, line charts, three dimensional charts, pictorial charts, and maps. The last third of the book presents samples of business charts. In presenting text and charts the authors do not discuss why something should be done in a certain way. They rarely present alternative methods of presentation; they only give the rules and leave it at that.

There can be little doubt that a simple book of this nature is badly needed today. The use of charts has been increasing rapidly and many businessmen need quick and simple guidance on how to draw charts. Unfortunately, "Practical Rules" cannot be recommended as filling this need. There are several reasons why it does not.

a) The proof of a chart is in its looks. The least we can expect from a book on graphic presentation is that the charts are well laid out, well designed, and fit harmoniously into the book. This is not the case in the book under review. Nearly all the charts look awkward; they lack proper balance within themselves and on the page. There is poor selection of size for lettering and poor weight selection for rulings and curves. The instructions on lettering sizes (p. 6) are insufficient, instructions on grid and curve weights (p. 34) limited to "light" and "heavy." Unnecessary or incorrect rules, as for instance, "that the grid will be wider than it is long in somewhat the same proportion as 5 is to 3" (p. 7) or that pictorial symbols should be "mere outlines with little detail" (p. 48) also make it more difficult to obtain clear and forceful charts.

b) This leads us to the second shortcoming of the book. Instructions are usually given as if there were "generally accepted standards" of chartmaking. While there are some such standards it is obvious to all of us who deal with charts that effective presentation calls for considerable flexibility. Most of us will make trial layouts before we draw up a chart. We do so because this permits us to make adjustments in the many different elements which go into a chart. We reorganize or eliminate some elements, we emphasize others, we adjust dimensions and sizes of lettering until we are satisfied that the chart is truly effective. Only then do we begin the final draft. If the authors had tried harder to make the draftsman understand what one can "get out"

of a chart by different treatments instead of giving a set of seemingly inflexible rules, a more helpful book would have resulted.

c) The authors state that they did not want to "give the pros and cons of various practices." (Actually they have done so occasionally displaying a preference for 100 per cent component bar charts over pie charts and absolute component bar charts.) It seems to this reviewer that an objective listing of the advantages and disadvantages of certain types would be essential in a practical book for those who cannot be expected to judge all the elements which go into making a chart correct and effective. Alternative ways of presenting the same set of data would also have been helpful. The wealth of samples included in Brinton's *Graphic Presentation* is a considerable help to the layman. It gives him a chance to select from different methods the one which fits his needs most clearly.

d) This reviewer has spent about twenty-five years in pioneering the development of pictorial presentation of statistical data. The shortcomings of the book under review naturally become most clearly obvious to him at this point. Pictographic presentation is advantageous in a limited field. These limitations in regard to audience, accuracy, and cost should be stated if we are to give a prospective chartmaker practical help. If the chartmaker decides for pictographs he will have to follow a set of simple rules; the authors give one of these and leave out all others. Several ways are available to duplicate pictorial symbols so that charts can easily be prepared. They can be photostated, photo-offset, type-stamped, or rubber stamped. Symbol sheets are sold by some organizations. None of this is mentioned in the book. The stencil suggested in the book is not being used by any chartmaker known to this reviewer. The statement that "pictorial charts are more effective if the classification is qualitative" is not correct. There is ample literature in existence on pictorial presentation to have avoided the textual and graphic shortcomings in this section.

e) A short book of this type needs a bibliography. Many readers will want to know where to turn for more information.

In 1926 the American Society of Mechanical Engineers formed a Sectional Committee on Standards for Graphic Presentation. In April 1936, the Society published a *Code of Preferred Practice for Graphic Presentation—Time-Series Charts.* On the basis of comments and suggestions received, a revision, *Time Series Charts—A Manual of Design and Construction,* was published in November 1938 with the approval of the American Standards Association. Nevertheless, the manual is not a compilation of rigid rules. The Committee believed that effective presentation calls for flexibility of treatment rather than for standardization, that each chart should be individually planned with due reference to the special characteristics of the data and the particular use to which the chart is put. Within these limitations there is now available a set of well-tested rules for the presentation of time-series charts. The authors could have performed a real service to students and businessmen if they had tried to popularize the more important elements

contained in the manual. At present this little volume, *Time-Series Charts*, is still the best guide for those who want to draw charts of the conventional type.

Control Charts: An Introduction to Statistical Quality Control. *Edward S. Smith* (Professor of Mathematics, University of Cincinnati, Cincinnati, Ohio), New York 18: McGraw-Hill Book Co., Inc. (330 West 42nd St.), 1947. Pp. xi, 161. $3.00. (London, W.C. 2: McGraw-Hill Publishing Co. Ltd. [Aldwych House, Aldwych]. 15*s*.) *Two reviews follow:*

REVIEW BY JOSEPH M. JURAN
Professor of Administrative Engineering and Chairman of the Department
New York University, New York, N. Y.

HERE is another of the numerous books now emerging to expound the use of statistical methods to control quality of product.

There is a good discussion of the use of control charts for averages, ranges, and number or fraction of defectives. Visual aids in the form of charts or tables are well prepared. The exercises are well chosen.

The claim that the book is "a test for the busy executive" is wishful thinking. In this work, and for that matter, in others, there is little evidence that the authors have truly grasped the quality problems of the industrial executive.

Industry's problem has been and still is to make product to meet specifications. The statistical methods, if they are to be grasped by industry, must be presented as a means to the solution of that problem. The emphasis must, in the first instance, be *not* on (*a*) how to achieve controlled processes; the emphasis must rather be on (*b*) the fact that through a controlled process it is easier to make products to meet specifications.

To a statistician, (*b*) seems only too obvious; ergo the executive should take (*b*) for granted, and thus only (*a*) remains to be learned. The precise opposite is true. The practical problem is to convince the executive of the truth of (*b*). Even after he is convinced of (*b*) he will ordinarily not be concerned with (*a*); instead he will delegate the learning of (*a*) to another.

With the exception of the unrealistic first chapter Professor Smith's book can well serve as a supplemental text for undergraduate engineers.

REVIEW BY MASON E. WESCOTT
Assistant Professor of Mathematics, Northwestern University
Evanston, Illinois

PROFESSOR SMITH has long been a successful teacher of college mathematics. During and since the war, he has participated as an instructor in numerous 8- and 10-day intensive training courses in statistical quality control for industrial people. He has conducted several successful in-plant training courses and has served as technical advisor for the installation and operation of statistical quality control programs. Drawing on this back-

ground of theoretical and practical experience, he has contributed *Control Charts* to the McGraw-Hill Industrial Organization and Management Series.

It has been the author's intention to write a text "for men and women who wish to acquire knowledge of the methods and charts of statistical quality control, but who are too busy to have an interest in the mathematical aspects of the methods." He believes that "it is more important than ever that everyone concerned with such production (production of goods of high quality) should be able to make and read control charts."

With the latter point of view, this reviewer is in complete agreement, but he does not feel that such understanding can be acquired without more exposure to things mathematical than the "busy executive" generally feels he can or wants to endure. As a matter of fact, the innocent reader who begins Professor Smith's book expectantly hoping to be painlessly initiated into the mysteries of statistical quality control, soon finds himself face to face with formulas and exposition that are pretty likely to appear frighteningly mathematical to him. That this is so, should not be held against the author, because Professor Smith has very nearly succeeded in keeping to a minimum the formal aspects of his subject. Rather, it is convincing testimony to the fallacy of trying to undertake an expository discussion of a technical subject, be it even so simple as control charts, without sooner or later having to use symbols and language that to a great many nonprofessional people must seem decidedly mathematical.

Chapter 1 is probably the best chapter in the book from the point of view of the audience to whom the book is addressed. Here the author introduces the definitions he proposes to follow for such concepts as "control," "quality," and "satisfactory control of quality," distinguishes well between inspection for the purpose of *controlling* quality and inspection which merely sorts good product from bad, outlines pointedly the fundamental role of a control chart as a criterion for telling when to take action and when to leave well enough alone, and lists ten benefits to be expected from a properly administered control chart operation. One may very well take exception to parts of the definitions given, but on the whole, they are acceptable for the purposes intended. The statement that "*A production process is said to be producing goods of satisfactory quality if 99.7 per cent of the items are satisfactory in quality*" is, to this reviewer, the kind of a statement he would rather not see used with the emphasis it is made to carry here.

The remaining chapters are, for the most part, well organized and clearly presented. One becomes conscious of a tendency to emphasize details rather than to paint the picture in broad outline. The latter treatment would have seemed more consistent with the objective of "selling" the subject to an audience which is not supposed to be interested in details. However, Professor Smith is understandably more the teacher than the salesman in these chapters, and as such, he anticipates and tries to remove the mental hazards that experience has taught him block the student's progress in learning how to make and use control charts. Certainly, if the reader will exercise the

patience and intellectual curiosity to study through these chapters, working the problems proposed as he goes, he will end up with at least the mechanical skill to make a control chart, and a somewhat better than bowing acquaintance with the operational meaning of such a chart.

In these chapters a number of technical points appeared questionable to this reviewer, but most of them are not serious enough to warrant detailed discussion here. Three points, however, cannot be ignored.

a) There is an irritating looseness with which the word "normal" is used. Such phrases as "normal, symmetrical distribution," "normal average," "normal and approximately symmetrical frequency distribution," "normal process" occur repeatedly without the benefit of any consistently adhered to definition; in fact, without any "definition" at all! Unless the reader has unusual insight or sophistication, he may very well end up with the happy delusion that everything will be just dandy if only it can be described as "normal." The word is simply overworked in the last four chapters without reserving to it the distinction it rightfully possesses in statistics.

b) It is the opinion of this reviewer that on pages 48–52, and elsewhere in subsequent discussions, Professor Smith overplays his hand in the matter of testing control against product specifications. In the first place, the practice of putting "danger limits" (three-sigma limits for individuals) on a chart for averages is questionable, particularly if the chart is to be kept at the machine. Exhibiting the to-be-expected spread for *individuals* on a chart for *averages* simply invites trouble, because the operator finds it hard to understand why he must keep the points within the narrower band set by control limits for averages when the "danger limits" are way out beyond this band.

In the second place, the discussion manages to convey the impression (although the contrary is explicitly stated on page 52) that there will frequently be hair-splitting closeness between a "danger limit" and a blueprint specification which has to be resolved before a controlled process can be adjudged in satisfactory control. The criterion described is not in error: it simply overemphasizes a relatively unimportant point. Time might better have been spent in showing how to estimate the per cent of production likely to be out of tolerance when control is not satisfactory, and in discussing more fully the problem of balancing the cost of sorting and/or rework against that of making process or specification changes that will yield control safely within acceptable blueprint tolerances once the optimum capabilities of the process have been determined.

c) The discussion in Chapter 4 uses the word "lot" where the word "sample" would ordinarily be preferable. The beginner reading this chapter must surely experience a sense of confusion when, after digesting 21 pages in which he has had the word "lot" drilled into him at every stage, he suddenly discovers on page 106 that a *box* full of beads is a "lot," but a *paddle* full of beads is a "sample," and that now it is the *sample* that is to be inspected 100 per cent rather than the *lot* as he has previously been led to expect! Evidently the author intended this chapter to deal with attribute inspection for

process control rather than for acceptance sampling, although, of course, the two are not mutually exclusive. But if inspection for process control is intended, it would seem more consistent and realistic to characterize a "day's production" (which the author says must be inspected 100 per cent!) as a "sample" from the cause system represented by the production stream than it is to describe these items as a "lot."

Furthermore, it would be entirely possible for the beginner to get the impression from this chapter that 100 per cent inspection (in the ordinary sense) is required to keep a fraction defective chart. Many plants exercise very satisfactory process control through the maintenance of fraction defective charts based on samples that are a very long way from being what the average man has in mind when he finds himself reading about 100 per cent inspection of a *lot*. It is the *sample* that is inspected 100 per cent, and it would be wiser to say so, rather than to use language that can be so easily misinterpreted.

Table II appears to be potentially useful in simplifying the calculations required for control limits on p and np charts. Its use is amply illustrated in Chapter 4.

The typography is good; tables, charts, and diagrams are well done and legible. The book is easy on the eyes. Except for a few missing punctuation marks, only two errors were found: on page vi, the name should be Paul Peach, and on page 142, the bar has been omitted over the R in the third line from the top.

Control Charts could prove useful as a text for a short training course at an elementary level. As a textbook for untutored home-study or as a "quickie" for the casual reader who wants to see the forest but not the trees, it is the opinion of this reviewer that the book is likely to prove disappointing.

Méthods Statistiques Modernes des Administrations Fédérales aux États-Unis. *Pierre Thionet.* Paris: Hermann & Co. (6 rue de la Sarbonne), 1946, Pp. 117.

REVIEW BY W. EDWARDS DEMING
Adviser in Sampling, Bureau of the Budget
Washington

THIS book is a unique and definite contribution toward improved statistical practice, as well as a delight to read. The author exhibits unusual ability in theory and extraordinary clarity in presentation. He recognizes the need for proper field organization in conducting sample surveys, and appreciates the need for reliable figures in government administration. The study of this book has been a pleasing experience for the reviewer. It is interesting to see how we are viewed from across the Atlantic, and gratifying to find the essential points of progress in the United States so clearly pointed out. Here, we are too close to give a coherent account of what has been happening. More-

over the reviewer feels a personal debt to Mr. Thionet for providing in one cover a clear exposition of much published research that was difficult in its original form.

The book was perhaps intended mainly as a digest of the essential mathematical papers that have made impact on statistical practice in the United States. Actually, it is much more than that. It is a manual as valuable to the nonmathematical administrator as to the student of theory. The author pleads for the use of figures in government administration in place of abstract theorizing and political maneuvering. Now that sampling has been developed to its present stage of reliability, real figures instead of fiction are possible and there is little excuse for operating without them. Progress is not achieved by shackling the theoretical statistician, but by giving him a chance. The author is fearless: "Et cette évolution de la méthologie statistique et même de la statistique mathématique risque de rester mal connue en France où les personnes (déjà peu nombreuses) qui connaissent et utilisent la méthode statistique, ne s'intéressent trop souvent qu'à l'un de ses aspects: pratique, théorique, mathématique, administratif, etc., alors que nous entrons dans un domaine où ils s'interpénètrent tous . . . En France, où d'ailleurs le nom même de statisticien, jadis équivalent de celui d'actuaire, a disparu de la hiérarchie administrative officielle, les conditions sont profondément différentes . . . Et l'ensemble de la nation fait preuve d'une prédilection marquée en faveur des idées abstraites de préférence aux réalitiés chiffrées, de sorte qu'un gouvernement ou une assemblée se préoccupera rarement de l'avis des statisticiens quand une décision doit être prise" (pp. 4–5).

This book is destined to be widely used. Fortunately a large part of the world reads French. With the coming census of population and agriculture, which most countries have agreed to take in 1950–51, there is an appalling need for sampling. The most serious handicap is official deafness, but lack of trained personnel and suitable texts is almost as serious. No one book will meet all needs, but every good one will help. In many countries, proper sampling procedure offers the only hope of obtaining census information of sufficient reliability and speed to be useful. Even in this country, with a well-developed census, sampling is used not only for current information on employment, housing, cost of living, characteristics of farms and farmers, and many other purposes, but was also used concurrently with the complete censuses of 1940 and 1945 (and will likewise be so used in 1950) with the aim of broadening the scope of the census and speeding up the returns.

The student of theory will find in this book an ably written and unified account of the mathematical researches that have guided progress in statistical practice. It is impossible to portray either its brilliant selection of contents or its lucidity in a review, but a hasty summary would include a nonmathematical introduction to the general principles of sampling, its advantages and disadvantages; descriptions of the U. S. Census tabulations and their use in sampling; appreciation of the fact that sampling is not only

an expedient of war-times but an instrument of stability in peace; some general theory and remarks on application. Then follow five chapters and two annexes, in which are beautifully expounded in uniform notation a number of methodological researches that have had impact on modern practice. Works of Hansen and Hurwitz, Neyman, the Madows, Cochran, Jessen, Stephan, Stock, Frankel, Geary, Hauser, Snedecor, King, H. Fairfield Smith, and Walter Hendricks are included. Unfortunately the author missed the profound researches of Mahalanobis, whose reports on sampling for the acreage of jute and other crops and conditions in Bengal have been recorded from time to time since 1937 if not earlier. Mahalanobis's magnificent article "On large-scale sample-surveys" (*Philos. Trans. Royal Soc.* 1944) probably reached Paris a bit late for inclusion: this article not only described Mahalanobis's brilliant researches in the use of grids in agricultural sampling, but also lays down the framework into which all sampling procedure must hereafter be fitted. This framework consists of four steps:

> *i.* Decide in advance on the widest permissible sampling tolerance, and specify also what measurements are desired on the differences between various alternative questionnaires, definitions, methods of training, and between interviewers, respondents, etc.
> *ii.* Design the cheapest plan that will meet these requirements and stay within the limitations of personnel and physical equipment available and any administrative restrictions that must be imposed. Compute the expected cost.
> *iii.* Reconsider Steps *i* and *ii* in view of the expected costs and precision. Perhaps relax the prescribed precision or forego some of the aims to cut costs; or tighten up the precision if the cost is not too great.
> *iv.* After the results of the survey are in, compute the costs and the sampling errors and the differences between the various alternative procedures that were specified. This appraisal of the results is independent of the assumptions that were made in the planning. It provides further knowledge of the proportions and variances in the universe, and of costs, so that a cheaper job can be done next time.

It is to be noted that these steps, which constitute modern statistical practice, were impossible and unthinkable until the theory of probability was introduced into the design of surveys and experiments. Thionet is clear on these points, although the steps as enumerated above are not stated explicitly.

The author himself has made a number of notable contributions in theory, not only by filling in gaps but by extension as well. By coincidence these extensions sometimes follow lines of unpublished recent researches that are even now profoundly modifying federal statistical practice. One such contribution is in the topic of *estimation*, in which the author does not shrink from the use of biased estimates. Given the simple case of Bernoulli sampling from each of L urns, three plans of estimating p are discussed from the standpoint of consistency, bias, and variance (pp. 28–30). If x_i is the number of black balls in a sample of n_i from the ith urn, three possible estimates are these:

1. $\hat{p} = \dfrac{\sum x_i}{\sum n_i}$

a. Consistent
b. Biased unless n_i is proportional to N_i
c. Var $\hat{p} = \dfrac{1}{n^2} \sum \dfrac{N_i - n_i}{N_i - 1} n_i p_i q_i$

2. $\hat{p} = \dfrac{1}{L} \sum \dfrac{x_i}{n_i}$

a. Inconsistent
b. Biased
c. Var $\hat{p} = \dfrac{1}{L^2} \sum \dfrac{N_i - n_i}{N_i - 1} \dfrac{p_i q_i}{n_i}$

3. $\hat{p} = \dfrac{\sum N_i \dfrac{x_i}{n_i}}{\sum N_i}$

a. Consistent
b. Unbiased
c. Var $\hat{p} = \dfrac{1}{N^2} \sum N_i^2 \dfrac{N_i - n_i}{N_i - 1} \dfrac{p_i q_i}{n_i}$

Then follows a discussion regarding comparisons of the three variances, with and without stratification, and under various systems of apportioning the sample amongst the strata. The author makes it clear that the aim in making an estimate is to come forth with a number whose reliability can be controlled and measured, whether it be biased or unbiased; that the procedure of estimation is part of the sampling plan; and that the cost of the sample and the precision of the results will depend on the procedure of estimation as well as on the method of selecting the households that are to be interviewed.

A review is not complete without one criticism. The reviewer fears that the section-heading on page 13 "Pas d'échantillonnage sans l'aide des recensements" will lead the uninitiated to suppose (as so many of the laity now suppose) that a sample must rely on a complete census for the total number of people; that the sample can only give estimates of the proportions of people in various classes, and that, even then, it is well to have some census information as a check. Information from a previous census is indeed useful in designing a sample, as Thionet points out, but it is not necessary. Clever use of available census information for stratification, for ratio-estimates, and for drawing areas with probability in proportion to previous size, will be effective in decreasing the standard error of subsequent samples, and hence in cutting costs. But census information is not necessary; detailed maps can be used for defining areal units and are often so used even when census information is available. Obsolete census information and obsolete directories, civil and parish lists, are useful for sampling because, with proper techniques of estimation, the errors in the prior information cancel out, leaving the sample estimates unbiased.

Mr. Thionet deserves the praise and thanks of all statisticians. The reviewer closes with the hope that translations of the book into Spanish and Chinese will appear in time for assistance in planning the world census of

population and agriculture in 1950–51 in places where French is difficult to read. An edition in English would likely find high favor as a text and reference book not only here but in other English-speaking countries, notably in countries like India where statistical teaching and practice have reached high levels.

An Extension Course in Statistical Quality Control. *A. E. R. Westman* (Director, Department of Chemistry, Ontario Research Foundation, Toronto 5, Canada). Reprint of series of articles which appeared in *Manufacturing and Industrial Engineering*. Toronto 1, Canada: Consolidated Press Ltd., 1947. Pp. 58. Paper. $1.25. *Two reviews follow:*

REVIEW BY A. C. COHEN, JR.
Associate Professor of Mathematics
University of Georgia
Athens, Georgia

THIS syllabus was originally prepared for use in statistical quality control courses which Dr. Westman conducted during the war under sponsorship of the Canadian Government for the benefit of Canadian industry. The Canadian courses were closely patterned after those offered in this country by OPRD for American war industry, but as reflected by the syllabus, the Canadian courses were even more condensed than the intensive OPRD courses.

Frequency distributions, control charts for measurements, control charts for counts, acceptance sampling with particular emphasis on the use of the Dodge-Romig tables, installation of a statistical quality control system, and other related topics are among the subjects discussed in the thirteen two-hour lessons which comprise the course. The various lessons include lectures to be presented by the instructor, demonstrations in which both the instructor and class members participate, and laboratory periods during which class members execute assigned exercises under the instructor's supervision. The numerous demonstrations which are described in detail effectively illustrate the theory of probability as it concerns statistical quality control, and the many exercises afford the student practice in applying the statistical techniques of quality control to practical situations.

This publication will unquestionably be a welcome addition to the quality control library of anyone concerned with the training of quality control personnel. It is particularly recommended to anyone confronted with the task of setting up and conducting a course of instruction in statistical quality control at the elementary level. Unfortunately its usefulness is marred by a somewhat lengthy list of errata. Most of the errata are typographical, but several involve formulas which if not corrected will certainly prove confusing and annoying to the uninitiated reader. A more clearly stated distinction between sampling for process control and sampling for acceptance (i.e. disposition) of product would be desirable. In this connection, although Lesson 12 is concerned with acceptance sampling, that fact is not explicitly stated, and

the reader having no previous knowledge of the subject will likely find the omission confusing. The subjects of correlation and contingency tables which are included in Lesson 13 may prove too advanced for a first course in quality control for anyone who has not had previous training in statistics, but if desirable, these topics can be omitted without detriment to other material in the course.

REVIEW BY PAUL PEACH
Associate Professor, Institute of Statistics
The University of North Carolina, Raleigh, N. C.

DURING the war, in connection with the 8-day courses in statistical quality control organized by the U. S. Office of Education and the War Production Board, there was prepared a study manual by Professors Holbrook Working and Edwin G. Olds which more or less stabilized the pattern of instruction for those courses. That manual was never published, but several of its legitimate children have attained the dignity of print. Dr. Westman's pamphlet contains an introduction in which this parenthood is specifically acknowledged.

The student begins by making some histograms and calculating a few means and standard deviations. Three following lectures deal with control charts for measurements, three more with the charts for p and c, and then three with acceptance sampling. The last three bear the titles "Installing a Quality Control System," "Other Sampling Inspection Schemes," and "Introduction to Advanced Topics."

The presentation is, I think, an improvement over previous ones; the style is clear, and each lecture is illustrated by diagrams, tables of data, and similar aids. A laboratory or demonstration period is provided for at the close of each meeting. Most of the chapters conclude with a list of special terms, though without definitions. Taking the work as a whole, one might say that for the most part it follows the pattern which was created in the years 1942 to 1944, treating the customary subjects in the established way, with just about enough novelty to justify a separate existence.

There may be some difference of opinion about these novelties. Dr. Westman includes less theoretical statistics than did the old WPB course, and I should be inclined to go even farther and eliminate theory altogether. The avowed viewpoint of the pamphlet is that statistical quality control is a branch of industrial engineering, and while I disagree with this idea, yet I agree entirely that a short course for engineers should skip the statistical theory. A noteworthy recent contribution to the literature is the new British Standards Institution publication *Fraction-Defective Charts for Quality Control* (B.S. 1313:1947) which has been almost completely purged of statistics.

Dr. Westman's final chapter treats control charts for composites, correlation, and the chi-square test. Presumably this chapter is intended as a sort of bonus, since it seems unlikely that any lecturer would attempt to cover

this much ground in a single period. Here again we may ask whether, when we can at best do so little, it would not be better to do nothing. Nevertheless, since the chapter comes at the end, the instructor can if he likes omit it, and no harm is done.

This last chapter means, I suppose, that engineers realize that the usual introduction to control charts and acceptance schemes is only a beginning, and that the value of other statistical tools is becoming recognized. Unfortunately, most of the present literature on statistical quality control ends in a blind alley; the student who wishes to learn advanced methods has to retrace his steps and start all over with new definitions, new concepts, and a new notation. In his first twelve chapters Dr. Westman follows the current writings on control charts, and of course is in a difficulty in his thirteenth. He cannot escape introducing the "degrees of freedom" concept, and gives formulas for the number of degrees of freedom appropriate to correlation coefficients and contingency tables; I am afraid that with this much introduction the concept will still seem pretty esoteric to his readers. There is room in our educational system for a manual that will do what Dr. Westman has done, but with a viewpoint and notation that will enable the student to study further if he likes without discarding most of his elementary instruction.

A word about typography may be in order. Most printed works, especially first printings, contain typographical errors; with these I am not concerned. Print shops not accustomed to mathematical text usually have trouble with it, especially with equations and other algebraic expressions which must be set by hand. Frequently, if special mathematical types are not available, it is necessary to use makeshifts. A lot can be done in this way if the author and the typesetter work closely together; but in the present work the handset portions are really a mess. Dr. Westman was also done dirt by in other ways; for instance, his publishers have not even bothered to use uniform style for chapter headings. The presswork and paper are good, but some of the editorial work is plain slovenly.

Toleranser och Passninger. *Stig Zetterlund.* Stockholm, Sweden: Sveriges Mekaniska Verkstäders Forbund, 1945.

Review by Cecil C. Craig
Professor of Mathematics, University of Michigan
Ann Arbor, Michigan

Of this rather compendious book on tolerances and fits written by a member of the staff of the famous Swedish manufacturer of ball bearings, the SKF company, only chapter 8 is of general interest to a statistical audience. To those who have devoted at least a share of their energy in recent years to advocating the use of statistical methods in the control of the quality of articles produced by repetitive processes, it is gratifying to see this much recognition given in a book like this to the importance of probability and

frequency laws, but it is also disappointing to see that the development here still stops well short of adequately conveying to those concerned with manufacturing interchangeable parts how useful statistical tools can be to them.

In this chapter the fact is brought out that the actual sizes of a part produced to the same nominal dimension are subject to variations which, if due to chance only, tend to be distributed in a bell-shaped frequency curve, very often usefully summarized by the normal frequency law, hence by means of the average and standard deviation. It is well illustrated that the play between two assembled parts, such as a hole and an axle, when each is subject to random variation, even of a rather arbitrary sort, tends to be so distributed. The chief problem actually dealt with is that of the variability to be expected in the sum of the dimensions of two assembled parts, when the variability of each is known.

Conference Papers: First Annual Convention, American Society for Quality Control and Second Midwest Quality Control Conference, Hotel Sherman, Chicago, Illinois, Thursday-Friday, June 5 and 6, 1947. Rochester 8, N. Y.: the Society (c/o Alfred L. Davis, Treas., Rochester Institute of Technology), 1947. Pp. vi, 284. Paper, lithotyped. $3.50. For a complete list of the papers and contributors in this volume, *see* 1431–2, 1438–9, 1453–5, 1528, 1562, 1650, 1665, 1691, 1712, 1714, 1718, 1720, 1761, 1767, 1770, 1776, 1781, 1791, 1847–8, 1853, and 1861 in "Statistical Methodology Index, No. 11" in the March 1948 issue of this JOURNAL. *Two reviews follow:*

REVIEW BY H. A. FREEMAN

Associate Professor of Statistics, Massachusetts Institute of Technology
Cambridge, Massachusetts

FIVE papers, three of them by corporation presidents, deal with broad aspects of quality control, such as the views of management and the problems of organization and administration of quality control programs. Seven papers, chiefly by Professors Burr of Purdue and Wescott of Northwestern, describe, quite nonmathematically, the apparatus of elementary statistical quality control. These include quality control charts for continuous and discrete variables, single and double sampling inspection for attributes, and the rudiments of linear correlation. Fifteen papers are devoted to descriptions of successful applications of elementary quality control to particular problems. Some of these deal with subjects to which the methods of statistical quality control are not often applied, for example, personnel data, time study, stockroom inventories, waste and personnel efficiency.

There remain two papers not classified above. One is a very good review of modern methods for variables inspection, including noncentral t, by Professor Olds of Carnegie Institute of Technology; from a statistical point of view it is the most interesting paper of all. The second paper, by Professor Peach of North Carolina, discusses the teaching of statistics to engineering students.

These are formative years for organized quality control and this is reflected in the proceedings of the first national conference. As the field begins to establish itself, it relies almost exclusively on a few elementary, easy-to-understand methods, almost all of which stem from the pioneering work of Shewhart, Dodge and others of the Bell Telephone Laboratories. More complicated, though perhaps not more effective, methods will undoubtedly come later. Another feature of this volume is its occasional evangelism. There will probably be less of this at later conferences for it seems certain that quality control methods will ultimately be very generally employed throughout American industry.

Finally, a word to would-be purchasers. This volume should interest anyone who is actually or potentially interested in industrial quality control. Administrators, plant personnel, teachers, and students will all find something in it for them. Its contents will not generally interest professional statisticians, except as they suggest problems which might be attacked by different methods.

The book is produced by the photo offset process and it is paper bound.

REVIEW BY EUGENE L. GRANT
Professor of Economics of Engineering, Stanford University
Stanford, California

ON JUNE 5 and 6, 1947, the second Midwest Quality Control Conference was held in Chicago in combination with the first annual convention of the American Society for Quality Control. Some 38 papers were presented dealing with statistical quality control applications in many different industries including radio and other electrical products, automobile parts, plastics, temperature controls, Scotch tape, pens and pencils, surgical dressings, beer, dairy products, veneer, cleansers, garments, chemicals, ammunition, pharmaceuticals, paper, hybrid corn, and the retail mail-order business. Through the energy of the Proceedings Committee of the Midwest Quality Control Conference, under the chairmanship of Professor Mason E. Wescott of Northwestern University, this volume containing twenty-nine of these papers has been made available to the quality control public. The volume, reflecting as it does current problems and current thinking in the quality control field, should be a useful addition to the library of any industrial statistician or any teacher of industrial statistics or statistical quality control.

A number of the papers are really expositions of principles of statistical method as applied to various industrial situations. These are at different levels of statistical sophistication. A group of five papers by Professor Wescott and Professor Irving W. Burr of Purdue University constitute a presentation of elementary control chart principles and the elements of acceptance sampling. Other papers, calling for greater familiarity with statistical methods on the part of the reader, deal with such topics as the use of modified

control limits, acceptance sampling by variables, statistical techniques for research and development, and the fundamentals of correlation.

Most of the papers, however, are descriptions of specific cases or discussions of technical statistical problems or managerial problems arising out of specific cases. The number of industries represented would seem to give an impressive answer to any industrial manager who gives the familiar response, "My business is different," to the suggestion that he look into the applicability of statistical quality control techniques in his own plant. Nevertheless, it should not be concluded from this spread of applications that the time has yet been reached when statistical quality control methods are in widespread use in American industry. In many cases the uses described may be only in a single plant in a large industry. Some of the authors make it clear that they feel that the surface has only been scratched in their organizations. In other papers where this is not specifically stated it is evident that, at least in June 1947 when these papers were presented, the applications were in the beginning stage.

Several of the papers deal with matters somewhat removed from strictly quality control problems. Two papers apply control charts to personnel statistics. Another applies statistical methods to the interpretation of time study data. A most interesting paper of this type is "Scientific Control of Stockroom Inventories" by R. H. Wilson, General Service Manager of Bell Telephone Laboratories; this applies probability mathematics to the determination of ordering points and presents a fresh and stimulating discussion which should be of interest to industrial engineers and cost accountants as well as to statisticians.

The final paper in the volume, "Sampling and Sorting Ammunition for the Attack on Normandy" by Col. Leslie E. Simon, should, in the opinion of this reviewer, become a classic in the quality control field. This is not only because of the spectacular nature of the subject matter but also because of the effective way in which Col. Simon includes good sound advice for quality control men on the nonstatistical matters associated with their jobs.

It is to be hoped that similar volumes of conference papers will be made available from subsequent A.S.Q.C. conventions and subsequent Midwest Quality Control Conferences.

STATISTICAL METHODOLOGY INDEX, NO. 12*

A QUARTERLY GUIDE TO CURRENT LITERATURE

Edited by

OSCAR KRISEN BUROS

Rutgers University

This bibliographic service presents statistical methodology literature (articles, books, theses, and chapters) published in 1945 to date. Anonymous references are listed first; other references follow in alphabetical sequence by first-named authors. Whenever known, the position and address of an author are given the first time his name appears as sole author or as the first listed of two or more authors; otherwise, his position and address are usually repeated for each of his publications. Volume number, issue number (in parentheses), pagination, and date of issue are given for each journal reference. Stars indicate books, theses, and pamphlets; asterisks indicate publications seen by the editor of this INDEX. *Authors of papers on statistical methodology are invited to send reprints to Oscar K. Buros, Rutgers University, New Brunswick, New Jersey in order to facilitate prompt and accurate listings. Foreign-language papers should be accompanied by English translations of titles. Information is desired about references omitted.*

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